

**Making the Science of Global Warming:  
A Social History of Climate Science in Britain**

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Doctor of Philosophy  
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2004

## **Declaration**

I hereby declare that this thesis is the result of my own work and has not, whether in the same or a different form, been presented elsewhere for any other degree or professional qualification. Information derived from the published or unpublished work of others has been acknowledged in the text and a list of references is given.

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## Abstract

Although both historians and sociologists have studied climate science, at least one major gap remains in the literature on the topic: the history of climate science in Britain. This study describes the development of climate science in Britain during the period from the 1950s to mid 1980s, with some comparative discussions of developments in the US and with particular respect to the topic of CO<sub>2</sub>-induced global warming. The study of climate and of its variations had traditionally been descriptive and regionally oriented, and regarded more or less as a minor branch of meteorology. With advances in numerical methods and computing technology, however, climate science was gradually transformed into a highly physics-based and mathematical science. By the late 1970s, climate science became dominated largely by dynamical meteorologists, atmospheric physicists and physical oceanographers, armed with complex physico-mathematical modelling as a principal methodology.

It was not that other approaches did not exist. In the 1960s and early 1970s, observational studies of climate such as those cultivated by Hubert Lamb, and simple climate modelling studies focusing on climate sensitivity and feedback processes, played a far more important role in raising the issues of climate change, both natural and anthropogenic. However, mainstream meteorologists and dynamical climatologists, who occupied a higher status within the cultural hierarchy of science, firmly believed that climate and its change could only be studied properly using numerical models of the large-scale atmospheric circulation – *i.e.*, general circulation models (GCMs). This cultural hierarchy constrained the way in which the modern science of global climate change was developed and the issue of CO<sub>2</sub>-induced global warming was understood and investigated. Such a tendency was particularly strong in Britain.

Nevertheless, the resulting GCM-based science of global warming was by no means homogeneous. This thesis argues that different institutional goals, different national political environments, different understandings of how to relate to policy, and the hierarchical relations between scientific subcultures all combined to produce different paths and styles of global warming research.

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## List of Abbreviations

AAAS	American Association for the Advancement of Science (US)
AEC	Atomic Energy Commission (US)
AGU	American Geophysical Union
AMS	American Meteorological Society
BAPMoN	Background Air Pollution Monitoring Network (WMO)
BRO	British Rainfall Organization
CEGB	Central Electricity Generating Board (UK)
CEQ	President's Council on Environmental Quality (US)
CFCs	chlorofluorocarbons
CIAP	Climatic Impact Assessment Program (US)
COMESA	Committee on Meteorological Effects of Stratospheric Aircraft (UK)
CRU	Climatic Research Unit, University of East Anglia (UK)
EBM	energy balance model
EPA	Environmental Protection Agency (US)
ERDA	Energy Research and Development Administration (US)
FAO	Food and Agricultural Organization
FCCC	Framework Convention on Climate Change
FEA	Federal Energy Administration (US)
FWCC	First World Climate Conference (WMO/ICSU/UNEP/FAO)
GARP	Global Atmospheric Research Programme (WMO/ICSU)
GATE	GARP Atlantic Tropical Experiment
GCM	general circulation model
GFDL	Geophysical Fluids Dynamics Laboratory (US)
GISS	Goddard Institute of Space Studies (NASA)
IAMAP	International Association of Meteorology and Atmospheric Physics
IAS	Institute for Advanced Study (US)
ICSU	International Council of Scientific Unions
IEA	Institute of Energy Analysis (US)
IGY	International Geophysical Year
IIASA	International Institute for Applied Systems Analysis
IMI	International Meteorological Institute (Sweden)
IMO	International Meteorological Organization (the predecessor of WMO)
INQUA	International Union for Quaternary Research
IPCC	Intergovernmental Panel on Climate Change (WMO/UNEP)
IUGG	International Union of Geodesy and Geophysics

JNWPU	Joint Numerical Weather Prediction Unit (US)
Met Office	British Meteorological Office
Met.O.13	Climatological Research Branch (Before 1963) Synoptic Climatology Branch (Since 1963)
Met.O.20	Dynamical Climatology Branch
MOHMAT	Met Office Historical Marine Air Temperature
MOHSST	Met Office Historical Sea Surface Temperature
NAS	National Academy of Sciences (US)
NCAR	National Center for Atmospheric Research (US)
NERC	Natural Environment Research Council (UK)
NOAA	National Oceanic and Atmospheric Administration (US)
NSF	National Science Foundation (US)
NWP	numerical weather prediction
ONR	Office of Naval Research (US)
ORNL	Oak Ridge National Laboratory (US)
OSTP	White House Office of Science and Technology Policy (US)
PSAC	President's Science Advisory Committee (US)
RCEP	Royal Commission on Environmental Pollution (UK)
RCM	radiative-convective model
RMS	Royal Meteorological Society (UK)
SCEP	Study of Critical Environmental Problems (US)
SCOPE	Scientific Committee on Problems of Environment (ICSU)
SDM	statistical-dynamical model
SMIC	Study of Man's Impact on Climate
SST	supersonic transport
UNCHE	United Nations Conference on the Human Environment
UNEP	United Nations Environmental Programme
UNESCO	United Nations Educational, Scientific, and Cultural Organization
USC-GARP	US Committee for GARP
USDOE	US Department of Energy
WCP	World Climate Programme (WMO/ICSU/UNEP)
WCRP	World Climate Research Programme (WMO/ICSU/UNEP)
WMO	World Meteorological Organization
WWW	World Weather Watch (WMO/ICSU)

## Acknowledgments

First and foremost, I would like to express my sincere respect and gratitude to my principal supervisor, Prof. Donald MacKenzie, not only for his clear and thoughtful guidance, but also for his constant support and encouragement. I am especially thankful for his patience regarding the completion of this thesis. I would also like to thank my second supervisor, Prof. David Bloor, for his invaluable insight and intellectual stimulation throughout my Edinburgh years. Together with Prof. MacKenzie, he has helped me to better understand and appreciate the complex nature of science and technology.

I am grateful to Prof. Robert S. Harwood at Edinburgh's Institute of Atmospheric and Environmental Science and Dr. Simon Shackley at the University of Manchester Institute of Science and Technology for agreeing, at short notice, to be my internal and external examiners respectively and for taking the time to read my thesis and make valuable suggestions.

My gratitude is extended to other faculty members, administrative staff and postgraduates at the Science Studies Unit, the Department of Sociology and the Research Centre for Social Sciences, who have provided me with a friendly social and academic atmosphere over the years. There are too many to mention individually, but particular thanks are due to Takuji Hara and Yuval Millo of the MacKenzie group, for their support and companionship, and to Mrs. Carole Tansley for her careful English proofreading and miscellaneous assistance.

I would like to thank my Korean friends in Edinburgh – Jinsang Hwang, Ki-Heung Kim, Hee-Cheol Yoon, and many others – for making my postgraduate life more interesting, fun and relaxed. I am most indebted to Jinsang and Ki-Heung, fellow students of Science and Technology Studies (STS), for their friendship and understanding. They have always put up with my incessant ramblings on various subjects, from STS to films, politics, and personal matters. Further thanks go to Jinsang's wife, Hayeon Cho, and his sister-in-law, Dayeon, for their hospitality at 36 Buccleuch Place.

During the course of my research and writing, the John Clarke Slater Fellowship from the American Philosophical Society enabled me to enjoy the social and intellectual life of

Harvard University. Prof. Everett Mendelsohn at the Department of the History of Science kindly supported my application for the Slater Fellowship and welcomed me to his department. Prof. Sheila Jasanoff at the Kennedy School of Government allowed me to participate in her STS seminars and gave me an informal affiliation in the later stage of my stay at Harvard. I thank them all heartily.

For oral history interviews and correspondence, I thank all the interviewees for their time and generosity and the Center for History of Physics of the American Institute of Physics and its Director, Dr. Spencer Weart, for providing me with a travel grant and helpful advice.

Thanks are also due to my friends, colleagues and senior scholars in Korea – Profs. Hwan-Suk Kim and Young-Hee Lee, Jae-Kak Han, Byoungyoon Kim, Myong-Jin Kim, Yang-Gu Kang (Center for Democracy in Science and Technology), Iroo Joo, Profs. Pil-Ryul Lee, Sungook Hong and Sang-Yong Song, Drs. Young-Joo Ko, Wi-Chin Song and Sungsoo Song (Korean STS Community), Dr. Jae-Gwang Won and Nam-Hyuk Hur (KDLP Environmental Policy Group) – for their inspiration, advice and support.

A very special word of thanks should go to my father, Dong-Hoon Kim, and my late mother, Chung-Choo Oh, for their endless love and support throughout my life. I am particularly grateful to my father, who has always encouraged me to pursue my academic interests and forgiven me for not being a dutiful son. This thesis is dedicated to him. I thank my brother, Kyu-Hyun Kim, and my sister-in-law, Young-Mi Park, who have helped me in many different ways. I also thank my father-in-law, Hee-Taek Shim and especially my mother-in-law, Jong-Ran Lee, for all the love and support they have given my wife and me.

Last but not least, I would like to express my deepest gratitude to my wife, Won Mok Shim, for her continued love, support, encouragement and patience, and for sharing my thoughts and feelings about life and everything else, through all our years together. Without her, this thesis would not have seen the light of day.



## Chapter 1. Introduction

In June 1988, over 300 scientists and policy-makers around the world gathered at the World Conference on “The Changing Atmosphere: Implications for Global Security” held in Toronto, Canada. The conference concluded that, in order to prevent human-induced global warming, the world’s carbon dioxide (CO<sub>2</sub>) emissions should be reduced by 20% from 1988 levels before the year 2005, thereby capturing international media attention.<sup>1</sup> Three months later, the British Prime Minister, Margaret Thatcher, who had not previously taken up environmental causes, surprised many by delivering a “green” speech to the Royal Society in London. She chose global warming and ozone layer depletion as the central theme, and stated: “we have unwittingly begun a massive experiment with the system of the planet itself.”<sup>2</sup> In her address to the UN General Assembly in November 1989, she declared that the most pressing task at the international level was “to negotiate a framework convention on climate change”.<sup>3</sup> These events coincided with the occurrence of an unusual heatwave and drought in the US and Europe. Temperature records also revealed the 1980s as the warmest decade in a century around the world.

The warming trend continued in the following years, and global warming soon developed into one of the most salient issues in international politics. At the Rio Earth Summit in June 1992, more than 160 nations agreed to curtail CO<sub>2</sub> emissions and signed up to the United Nations Framework Convention on Climate Change (FCCC).<sup>4</sup> Parties to the FCCC met five years later in December 1997 at the World Climate Summit in Kyoto, where the Protocol was formally adopted to set legally enforceable targets for reducing global emissions of CO<sub>2</sub> and other greenhouse gases.<sup>5</sup> If approved, many industrialized countries would be required to meet the targets of an average 5 % below 1990 levels by between 2008 and 2012. As the US refusal to ratify the Protocol demonstrates, the

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1 **WMO/UNEP** *The Changing Atmosphere - Implications for Global Security* (Geneva: WMO, 1989)

2 **M. Thatcher** *Speech to the Royal Society* (September 27, 1988)

3 **M. Thatcher** *Speech to United Nations General Assembly* (November 8, 1989)

4 **M. Paterson** 'Before and After the Rio - Interstate Negotiations', in *Global Warming and Global Politics* (London: Routledge, 1996): Chapter 3.

5 **M. Grubb et al.** *The Kyoto Protocol: A Guide and Assessment* (London: RIIA/Earthscan, 1999)



apparent international policy consensus turned out to be fragile. Even so, the formulation of the Kyoto Protocol was a remarkable achievement. A 5 % reduction target was significant because, without action, greenhouse gas emissions were projected to continue to increase. The shift away from fossil fuel energy would necessitate a radical restructuring of the global economy. Some of the world's largest industries were expected to be hit severely.

How did the Kyoto Protocol come into being in the face of immense political and economic pressures? How was it that, in a country that was often criticized for being “The Dirty Man of Europe” in the early to mid 1980s,<sup>6</sup> a conservative, business-friendly Prime Minister suddenly became environmentally conscious, calling for the international convention on global climate change? Amongst the factors involved was the unprecedented share of the vote (14.9 %) achieved by the Green Party in the European election in Britain in 1989, which shocked the more established parties.<sup>7</sup> But, as the literature in sociology of science and environmental sociology suggests, a successful recognition and dissemination of environmental issues needs to be underpinned by science.<sup>8</sup> This is even more the case for global environmental issues, since the very identification of their “global” nature across different parts of the earth would not be possible without the supposedly universal validity of science.<sup>9</sup>

A few days before the Toronto climate conference, for instance, James Hansen, a climate scientist at NASA's Goddard Institute of Space Studies (GISS), testified at the

6 **M.A. Levy** 'European Acid Rain: The Power of Tote-Board Diplomacy', in *Institutions for the Earth: Sources of Effective International Environmental Protection*, P.M. Haas *et al.* (eds) (Cambridge: MIT Press, 1993): 73-132.

7 This result was, and still is, the highest national percentage vote of any Green Party in Europe. See, e.g., **M.N. Franklin and W. Rudig** 'On the Durability of Green Politics - Evidence from the 1989 European Election Study', *Comp. Polit. Stud.* **28** (1995): 409-39; **W. Rudig et al.** 'Up and Down with the Greens - Ecology and Party politics in Britain, 1989-1992', *Elect. Stud.* **15** (1996): 1-20.

8 See, e.g., **U. Beck** *Risk Society: Towards a New Modernity* (London: Sage, 1992); **T. Benton and M. Redclift** (eds) *Social Theory and the Global Environment* (London: Routledge, 1994); **R.E. Dunlap and W.M. Michelson** (eds) *Handbook of Environmental Sociology* (London: Greenwood Press, 2002); **M.A. Hajer** *The Politics of Environmental Discourse: Ecological Modernization and the Policy process* (Oxford: Clarendon, 1995); **J.A. Hannigan** *Environmental Sociology: A Social Constructionist Perspective* (London: Routledge, 1995); **A. Irwin** *Sociology and the Environment: A Critical Introduction to Society, Nature and Knowledge* (Cambridge: Blackwell, 2001); **S. Jasanoff** *The Fifth Branch: Science Advisors as Policymakers* (Cambridge: Harvard University Press, 1990); **S. Lash et al.** (eds) *Risk, Environment and Modernity: Towards a New Ecology* (London: Sage, 1996); **M. Redclift and G. Woodgate** (eds) *The International Handbook of Environmental Sociology* (Cheltenham: Edward Elgar, 1997); **S. Yearley** *The Green Case: A Sociology of Environmental Issues, Arguments and Politics* (London: Unwin Hyman, 1991); *Sociology, Environmentalism, Globalization: Reinventing the Globe* (London: Sage, 1996)

9 For this point, see **Benton and Redclift** (eds) *Social Theory and the Global Environment*; **Yearley** *Sociology, Environmentalism, Globalization*; **S. Jasanoff and B. Wynne** 'Science and Decision Making', in *Human Choice and Climate Change*, S. Rayner and E.L. Malone (eds), Vol 1. The Societal Framework (Columbus: Battelle

Hearing of the US Senate Committee on Energy and Natural Resources. Having been requested to provide his expert opinion on the problem of global climate change, Hansen told the Senate Committee that he had “99 percent confidence that current temperatures represent a real warming trend rather than a chance fluctuation”.<sup>10</sup> He went on to link the temperature trend to the increase in atmospheric CO<sub>2</sub> levels, and claimed that “the greenhouse effect has been detected, and it is changing our climate now”.<sup>11</sup> Hansen’s bold testimony was promptly reported in major newspapers, further helping to popularize the CO<sub>2</sub> issue.<sup>12</sup> His statement was, in fact, stronger than the view held by many of his colleagues, and some saw it as not cautious enough. Nevertheless, his use of sophisticated global climate models was highly regarded within the climate science community.

Indeed, many observers point out, it was the emergence of a new science of global climate change that eventually forced the political leaders of industrialized countries to agree on the need for policy action.<sup>13</sup> The study of climate and its variations had traditionally been regionally oriented, and considered merely as a minor branch of meteorology – *i.e.*, descriptive endeavours devoted to the compilation of local meteorological observations.<sup>14</sup> In the first half of the 20<sup>th</sup> century, the scientific status of the umbrella discipline of meteorology itself was in doubt. During and after WWII, the situation began to change. Meteorological sciences were increasingly transformed into physics-based and mathematical sciences, bringing about the birth of dynamical meteorology.<sup>15</sup> By the late 1970s, climate science also became more or less dominated by

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Press, 1998): 1-87.

10 **J.E. Hansen** 'Prepared Statement', in *Greenhouse Effect and Global Climate Change - Hearings before the Committee on Energy and Natural Resources, US Senate (June 23)* (Washington, DC: US GPO, 1988): 42-79.

11 Ibid.

12 See **Anon.** 'Global Warming Has Begun: Expert Tells Senate', *New York Times* (June 24, 1988); 'His Bold Statement Transforms the Debate On Greenhouse Effect', *New York Times* (August 23, 1988). See also **S.H. Schneider** 'Mediarology', in *Global Warming - Are We Entering the Greenhouse Century?* (San Francisco: Sierra Club, 1989): Chapter 7.

13 See, for example, **S.H. Schneider** *Global Warming - Are We Entering the Greenhouse Century?* (San Francisco: Sierra Club, 1989); **W.W. Kellogg** 'Theory of Climate Transition from Academic Challenge to Global Imperative', in *Greenhouse Glasnost - The Crisis of Global Warming*, T.J. Minger (ed) 1<sup>st</sup> edition (New York: Ecco Press, 1990): 99-121; **B. Bolin** 'Science and Policy-Making', *Ambio* **23** (1994): 25-29; **R.G. Fleagle** *Global Environmental Change - Interactions of Science, Policy, and Politics in the United States* (Westport: Praeger, 1994)

14 **F. Nebeker** 'An Empirical Tradition - Climatology', in *Calculating the Weather - Meteorology in the 20th Century* (New York: Academic Press, 1995): Chapter 2; **S.R. Weart** 'Climatology as a Profession', in *The Discovery of Global Warming*, <http://www.aip.org/history/climate> (recorded copy at American Institute of Physics, 2001): p. Climatology-1-12 of 08/01/01 text.

15 *Dynamical (or dynamic) meteorology* refers a branch of meteorology, which studies atmospheric motions as solutions of the fundamental equations of hydrodynamics or other systems of equations appropriate to special situations. See **F. Nebeker** *Calculating the Weather - Meteorology in the 20th Century* (New York: Academic

dynamical meteorologists, atmospheric physicists and physical oceanographers, armed with global climate modelling as a principal methodology.

For the last decade or so, because of its enormous impact on human society, this new science of global climate change has received considerable attention from social and political scientists. And yet, most of the studies by these scholars have been concerned with the role of science in global warming politics and policy-making.<sup>16</sup> They have treated the science itself as a black box: science is assumed to be driven by an inner logic, and the current form is accepted as the best available – if not inevitable – outcome. Even when the Whiggish account of science is rejected, and the historically contingent and context-bound nature of climate scientific knowledge is explicitly problematized, the analysis tends to be restricted to the contemporary era. The history of global warming research is touched upon only as a brief background.<sup>17</sup> More detailed historical investigations into the subject do exist and are growing in number, as illustrated for example by the work of Edwards, Weart, and Fleming.<sup>18</sup> However, they have concentrated mainly on the US case. The study

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Press, 1995)

16 See, e.g., **S. Boehmer-Christiansen** 'Global Climate Protection Policy - The Limits of Scientific Advice, Part 1 & 2', *Global Environ. Change* **4** (1994): 140-59, 85-200; 'Britain and the Intergovernmental Panel on Climate Change: The Impacts of Scientific Advice on Global Warming - Part 1 & 2', *Environmental Politics* **4** (1995): 1-18, 175-96; **I.H. Rowlands** *The Politics of Global Atmospheric Change* (Manchester: Manchester University Press, 1995); **T. O'Riordan and J. Jäger** *The Politics of Climate Change - A European Perspective* (London: Routledge, 1996); **M. Paterson** *Global Warming and Global Politics* (London: Routledge, 1996); **R.D. Brunner** 'Policy and Global Change Research - A Modest Proposal', *Climatic Change* **32** (1996): 121-47.

17 See **B. Wynne** 'SSK's Identity Parade - Signing-Up, Off-and-On', *Soc. Stud. Sci.* **26** (1996): 357-91; **S. Shackley and B. Wynne** 'Global Climate Change - The Mutual Construction of an Emergent Science-Policy Domain', *Science and Public Policy* **22** (1995): 218-30; 'Representing Uncertainty in Global Change Science and Policy - Boundary-Ordering Devices and Authority', *Sci. Technol. Hum. Values* **21** (1996): 275-302; 'Global Warming Potentials - Ambiguity or Precision as An Aid to Policy?' *Clim. Res.* **8** (1997): 89-106; **S. Shackley et al.** 'Uncertainty, Complexity and Concepts of Good Science in Climate Change Modelling - Are GCMs the Best Tools?' *Climatic Change* **38** (1998): 159-205; 'Adjusting to Policy Expectations in Climate Change Modeling - An Interdisciplinary Study of Flux Adjustments in Coupled Atmosphere-Ocean General Circulation Models', *Climatic Change* **43** (1999): 413-54. See also **P.N. Edwards** 'Global Comprehensive Models in Politics and Policymaking', *Climatic Change* **32** (1996): 149-61; 'Global Climate Science, Uncertainty and Politics: Data-laden Models, Model-filtered Data', *Sci. Cult.* **8** (1999): 437-72; **D. Sarewitz and R. Pielke** 'Prediction in Science and Policy', *Technol. Soc.* **21** (1999): 121-33.

18 For the early attempts, see **D.G. Victor and W.C. Clark** *The Greenhouse Effect in the US - A History of the Science Up To 1985*, Contribution I-2 on the project on Social Learning in the Management of Global Environmental Risks (Cambridge: Kennedy School of Government, Harvard University, 1991); **D.M. Hart and D.G. Victor** 'Scientific Elites and the Making of US Policy for Climate Change Research, 1957-74', *Soc. Stud. Sci.* **23** (1993): 643-80. The three major historians in this field are P.N. Edwards, S.R. Weart, and J.R. Fleming. For the work of Edwards and Weart, see **P.N. Edwards** 'The World in a Machine - Origins and Impacts of Early Computerized Global Systems Models', in *Systems, Experts, and Computers*, T.P. Hughes and A.C. Hughes (eds) (Cambridge: MIT Press, 2000); **P.N. Edwards and M.H. Lahsen** 'Climate Science and Politics in the United States', in *Planetary Management and National Political Cultures*, P.N. Edwards and C. Miller (eds) (Cambridge: MIT Press, forthcoming), and **S.R. Weart** 'Global Warming, Cold War, and the Evolution of Research Plans', *Hist. Stud. Phys. Biol.* **27** (1997): 319-56; 'Climate Change, Since 1940', in *Sciences of the Earth - An Encyclopedia of Events, People, and Phenomena*, G.A. Good (ed) (New York: Garland Publishing Inc., 1998); *The Discovery of Global Warming*, <http://www.aip.org/history/climate> (recorded copy at American Institute of Physics, 2001). Fleming is working on the earlier part of the story. See **J.R. Fleming** *Historical*

in this thesis aims to fill in a major gap in the previous studies so far: the history of the science of global climate change in Britain.

It is well recognized that, since the mid to late 1980s, Britain has been one of the key players in the science of global warming. In 1988, Sir John Houghton, director-general of the British Meteorological Office (hereafter, the Met Office), was appointed to chair the scientific assessment working group of the Intergovernmental Panel on Climate Change (IPCC), which was established by the World Meteorological Organization (WMO) and the United Nations Environmental Programme (UNEP) to advise governments on the state of knowledge of climate change.<sup>19</sup> Later in 1990, with the Thatcher government's support, the Met Office and the Department of Environment jointly set up the Hadley Centre for Climate Prediction and Research. The Centre formed a technical support unit for the IPCC's scientific working group, assisting the publication of the first IPCC Scientific Assessment report.<sup>20</sup> Scientists at the Climatic Research Unit (CRU), University of East Anglia, also made important contributions to this effort. The report subsequently acted as a catalyst in mobilizing international political support for the FCCC.

On the other hand, little is currently known about British climate science prior to the mid 1980s. In the present thesis I have explored how, up to 1985, the modern science of global climate change evolved in Britain. It may be noted that the choice of the year 1985 is somewhat arbitrary and more to do with the limitations of the information that I was able to collect and access, although the work of British climate scientists came to be more visible internationally around this time. The study is primarily historical, but does not intend to be comprehensive. The science of global climate change is bound to be multi-disciplinary, spanning diverse scientific traditions. To follow the trajectories of all these traditions would be a daunting task. Instead, I have selected certain aspects of British

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*Perspectives on Climate Change* (Oxford: Oxford University Press, 1998); 'Climate Change, Before 1940', in *Sciences of the Earth - An Encyclopedia of Events, People, and Phenomena*, G.A. Good (ed) (New York: Garland Publishing Inc., 1998); 'Joseph Fourier, the "Greenhouse Effect", and the Quest for a Universal Theory of Terrestrial Temperatures', *Endeavour* **23** (1999): 72-75; 'T. C. Chamberlin, Climate Change, and Cosmogony', *Stud. Hist. Philos. Mod. Phys.* **31B** (2000): 293-308.

19 The IPCC has three Working Groups: (a) Working Group I assesses the scientific aspects of the climate system and climate change; (b) Working Group II assesses the vulnerability of socio-economic and natural systems to climate change, negative and positive consequences of climate change, and options for adapting to it; and (c) Working Group III assesses options for limiting greenhouse gas emissions and otherwise mitigating climate change. For the history of the IPCC, see **S. Agrawala** 'Context and Early Origins of the Intergovernmental Panel on Climate Change', *Climatic Change* **39** (1998): 605-20.

20 **J.T. Houghton et al.** *Climate Change - The IPCC Scientific Assessment* (Cambridge: Cambridge University Press, 1990)



climate research that are crucial with respect to the global warming hypothesis.

This does not mean that the history of climate science in Britain can be considered separately from that in other countries. After all, the very issue of global climate change goes beyond national boundaries, and has attracted scientists from many different countries. There has been an extensive international exchange of information, knowledge, ideas and experiences in the field, especially in the post-World War II period. With more abundant manpower, funding, and technological resources, the US has played a particularly important role in this development, both scientifically and politically. Hence, throughout this thesis, I have also attempted to review and examine the history of the science and politics of global climate change in the US, often in detail.

Many of the relevant archival documents are, unfortunately, not yet open to public access. The main source material for the study has been, therefore, the scientific literature from a range of disciplines such as climatology, meteorology, physical oceanography, geology, palaeontology, astronomy and physical geography. Equally important source materials were internal technical reports, memoranda, and other documents deposited at the Met Office Library and Archive. I am grateful for the help of the staff at the Library in searching and obtaining technical notes and other documents. I have also used technical reports from several national and international organizations, including WMO. Additional materials used range from annual reports of government agencies, conference proceedings, workshop reports, scholarly books, and reports and articles in newspapers and scientific magazines, to popular science writings of other forms. The examination of these sources was complemented by oral history interviews, conducted by myself and others, with British as well as US scientists who had participated in climate research.

The climate science of the 20<sup>th</sup> century is esoteric in nature. Analysts who do not have appropriate background knowledge would find it difficult to understand its technical contents accurately, but even if successful, they would have to rely on the same skills employed by the scientists whose cultures are under study. To alleviate this interpretative problem, in some cases, I have adopted a more “charitable” understanding of those climatological studies that were relatively less valued by the standards and perspectives of

mainstream climate scientists.<sup>21</sup> In other cases, however, I have followed, rather than problematized, the actors' accounts as they were.

This historical inquiry has nonetheless been informed by sociological insights. As many historical and sociological studies of science have convincingly shown, the processes by which scientific theories and practices are generated and evaluated are themselves social, and thus subject to sociological scrutiny.<sup>22</sup> Such processes, these studies argue, are simply determined by nature, logic or impersonal methods, but are embedded in the routines of scientific sub-disciplines and in their intellectual traditions. In other words, the very contents of scientific knowledge and of scientific practices are shaped by the goals, values, interests, skills and resources of the scientific groups and communities involved, which may also be influenced by social factors outside science. I believe that the same argument can be applied to the present study. My goal here, though, is more modest than to produce

21 For the use of charitable interpretation in history and sociology of science, see **S. Shapin and S. Schaffer** *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985): Chapter 1.

22 See, e.g., **B. Barnes and S. Shapin** (eds) *Natural Order: Historical Studies of Scientific Culture* (London: Sage, 1979); **B. Barnes and D. Edge** (eds) *Science in Context: Readings in the Sociology of Science* (Milton Keynes: Open University Press, 1982); **B. Barnes et al.** *Scientific Knowledge: A Sociological Analysis* (Chicago: University of Chicago Press, 1996); **D. Bloor** *Knowledge and Social Imagery* (Chicago: University of Chicago Press, 1991); **D. Edge and M. Mulvey** *Astronomy Transformed: The Emergence of Radio Astronomy in Britain* (London: John Wiley & Sons, 1976); **D.A. MacKenzie** *Statistics in Britain 1865-1930: The Social Construction of Scientific Knowledge* (Edinburgh: Edinburgh University Press, 1981); *Inventing Accuracy: An Historical Sociology of Nuclear Missile Guidance* (Cambridge: MIT Press, 1993); *Knowing Machines: Essays on Technical Change* (Cambridge: MIT Press, 1996); *Mechanizing Proof: Computing, Risk, and Trust* (Cambridge: MIT Press, 2001); **S. Shapin** 'History of Science and Its Sociological Reconstructions', *Hist. Sci.* **20** (1982): 157-211; *A Social History of Truth: Civility and Science in Seventeenth Century England* (Chicago: University of Chicago Press, 1994); **Shapin and Schaffer** *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*; **J. Harwood** *Styles of Scientific Thought: The German Genetics Community, 1900-1933* (Chicago: University of Chicago Press, 1993); **L. Fleck** *Genesis and Development of a Scientific Fact* (Chicago: University of Chicago Press, 1979); **A. Pickering** *Constructing Quarks: A Sociological History of Particle Physics* (Edinburgh: Edinburgh University Press, 1984); *Science as Practice and Culture* (Chicago: University of Chicago Press, 1992); **H.M. Collins** *Changing Order: Replication and Induction in Scientific Practice* (Chicago: University of Chicago Press, 1985); *Artificial Experts: Social Knowledge and Intelligent Machines* (Cambridge: MIT Press, 1990); **T.J. Pinch** *Confronting Nature: The Sociology of Solar-Neutrino Detection* (Dordrecht: Reidel, 1986); **B. Latour** *The Pasteurization of France* (Cambridge: Harvard University Press, 1988); **B. Latour and S. Woolgar** *Laboratory Life: The Construction of Scientific Facts* (Princeton: Princeton University Press, 1986); **M.J.S. Rudwick** *The Great Devonian Controversy: The Shaping of Scientific Knowledge Among Gentlemanly Specialists* (Chicago: University of Chicago Press, 1985); **D.J. Haraway** *Primate Visions: Gender, Race, and Nature in the World of Modern Science* (London: Routledge, 1989); **D. Gooding et al.** (eds) *The Uses of Experiment: Studies in the Natural Sciences* (Cambridge: Cambridge University Press, 1989); **S. Traweek** *Beamtimes and Lifetimes: The World of High Energy Physicists* (Cambridge: Harvard University Press, 1988); **J.H. Fujimura and A.E. Clarke** (eds) *The Right Tools for the Job: At Work in Twentieth Century Life Sciences* (Princeton: Princeton University Press, 1992); **J.H. Fujimura** *Crafting Science: A Sociohistory of the Quest for the Genetics of Cancer* (Cambridge: Harvard University Press, 1996). For somewhat different but contextualized accounts of science, see **J.Z. Buchwald** (ed) *Scientific Practice: Theories and Stories of Doing Physics* (Chicago: University of Chicago Press, 1995); **P.L. Galison and D.J. Stump** (eds) *The Disunity of Science: Boundaries, Contexts and Power* (Stanford: Stanford University Press, 1996); **P.L. Galison** *How Experiments End* (Chicago: University of Chicago Press, 1987); *Image and Logic - A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997); **I. Hacking** *Representing and Intervening* (Cambridge: Cambridge University Press, 1983)

a full-fledged historical sociology of British climate science. While I have dealt with questions that are essentially sociological in the discussion chapter, I have generally focused on constructing a narrative history.

The thesis comprises of six chapters. The rest of this introduction presents a short pre-history of the science of global warming for the period up to the mid 20<sup>th</sup> century. Scientific research on the connection between climate and atmospheric CO<sub>2</sub> has a long history, and some of this history is very briefly reviewed.

Chapter 2 gives a historical account of new approaches to the study of climate. During the early to mid 20<sup>th</sup> century, physico-mathematical methods began to be fully applied to meteorology. Along with advances in electronic computers and the expansion of the upper-air observation network, this resulted in a very different kind of meteorology, in which numerical modelling of the atmosphere was a vital component. The growth of numerical meteorology, in turn, prompted the formation of dynamical climatology, at the Met Office and elsewhere, and led to the advent of general circulation models (GCMs), that is, numerical models of large-scale atmospheric circulation.

Initially, other lines of research were instrumental in raising awareness of climate change. One notable example was Hubert Lamb's historical climatology. Chapter 3 begins with Lamb's efforts to cultivate historical climatology at the Met Office. Later, dynamical climatologists quickly gained intellectual hegemony within climate science, but were preoccupied with developing GCMs. I show how these different approaches interacted with each other, and responded to a growing public concern about the issue of climate change, both natural and anthropogenic, from the 1960s to the mid 1970s.

Chapter 4 returns to the early 1950s, when scientific interest in the CO<sub>2</sub> theory of climate change was revived in the US and Sweden. Again, in the beginning, many of those who had been active in fostering global warming research were not involved in GCM work. Gradually, however, global warming research was taken over by GCM-based climate science. I outline some of these processes, and discuss why such developments were largely absent in Britain. I also show that the framing of CO<sub>2</sub>-induced global warming, first as a pollution issue and later as an energy problem, was important in drawing the attention of scientists to the subject.

Chapter 5 concerns the way in which the GCM-based science of global warming came to be established in Britain. Unlike in the US, the topic of CO<sub>2</sub>-induced global warming continued to be unpopular in Britain. The Met Office senior scientists had shown little interest in the climatic effects of CO<sub>2</sub>. Lamb, who had left the Met Office to set up CRU, was also sceptical of the global warming hypothesis. As the CO<sub>2</sub> issue achieved greater scientific recognition in the late 1970s, the Met Office and CRU initiated global warming research. I describe how these research programmes were further elaborated in the early to mid 1980s.

The final chapter pulls together the threads of historical accounts from the previous chapters. The development of climate science during the period from the 1960s to the mid 1980s was constrained by a cultural hierarchy of science that privileged complex physico-mathematical modelling. Yet the resulting GCM-based science of global warming was by no means homogeneous. I argue that different institutional goals, different national political environments, different understandings of how to relate to policy, and the hierarchical relations between scientific subcultures all combined to produce different paths and styles of global warming research.

### **Brief Pre-History of the Science of Global Warming**

The basic principle of the greenhouse effect is straightforward.<sup>23</sup> The earth receives heat from the sun in the form of short-wave radiation, which is absorbed by the earth's surface and emitted back to the atmosphere as long-wave thermal radiation. It is believed that, in the absence of another heat transfer mechanism, the energy balance would result in an average surface temperature on the earth of *ca.* -18 °C. Whereas many atmospheric trace gases are transparent to the incoming solar radiation, some of them – primarily water vapour and CO<sub>2</sub>, but also methane (CH<sub>4</sub>), nitrous oxide (N<sub>2</sub>O), ozone (O<sub>3</sub>), and others – are understood to absorb the outgoing thermal radiation and re-radiate heat in all directions. The resultant additional heat partly travels back down to the earth's surface, and gives rise to the present habitable state with a mean temperature of *ca.* 15 °C. This process resembles the warming of air in a greenhouse, and is therefore usually referred to

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<sup>23</sup> For a good introduction to the science of the greenhouse effect, see, *e.g.*, K. Warr and S. Smith *Science*



as the “greenhouse effect”. Even though some scientists argued that the metaphor of “greenhouse” did not accurately capture the role of convection, there has been a general consensus on the “natural” greenhouse effect.<sup>24</sup> The real question is whether this effect would be enhanced by the build-up of anthropogenic CO<sub>2</sub> in the atmosphere, further warming up the globe.

The idea of anthropogenic climate change is not new. In the 18<sup>th</sup> century, for instance, Enlightenment thinkers claimed that the European colonization of North America could alter regional-scale climate changes through cultivation and clearing of the forests.<sup>25</sup> Despite recurrent speculations about human impact on climate throughout the human history, however, scientific investigations on the subject were rare. The breach came from a somewhat different direction. In the mid 19<sup>th</sup> century, Louis Agassiz, a Swiss geologist, proposed the theory of Ice Ages. His work, and subsequent research by James Croll in Scotland, inspired many scientists from various disciplines to theorize about the causes of climate change.<sup>26</sup> One of them was John Tyndall, an Irish physicist who was exploring the radiative properties of water vapour and carbon dioxide in the atmosphere. He conjectured that the changes in the constitution of the earth’s atmosphere might have been the cause of the glacial period.<sup>27</sup>

Tyndall’s argument was further developed in 1896 by Svante Arrhenius, a Swedish physical chemist. Unlike Tyndall, he believed that water vapour would not make a significant contribution, and he focused on the radiative effects of CO<sub>2</sub>. By using spectroscopic information, he undertook lengthy calculations of the effects of atmospheric CO<sub>2</sub> on surface temperature, and suggested that the increase of CO<sub>2</sub>, blocking thermal radiation, would lead to a warming of the earth. He estimated that a doubling of CO<sub>2</sub> concentrations would yield an annual average warming of about 5–6 °C,<sup>28</sup> not very far off

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*Matters: Changing Climate* (Milton Keynes: Open University Press, 1993)

24 For this debate, see **R. Lee** ‘Greenhouse Effect’, *J. Appl. Meteorol.* **12** (1973): 556-57; **E.X. Berry** ‘Comments’, *J. Appl. Meteorol.* **13** (1974): 603-04.

25 **J.R. Fleming** ‘Climate and Culture in Enlightenment Thought’ and ‘The Great Climate Debate in Colonial and Early America’, in *Historical Perspectives on Climate Change* (Oxford: Oxford University Press, 1998): Chapter 1 & 2.

26 For the work of Agassiz and Croll, see **J. Imbrie and K.P. Imbrie** *Ice Ages: Solving the Mystery* (Cambridge: Harvard University Press, 1986)

27 **J. Tyndall** ‘On the Absorption and Radiation of Heat by Gases and Vapours, and on the Physical Connection of Radiation, Absorption, and Conduction’, *Philos. Mag.* **Ser. 4**, **22** (1861): 169-94, 273-85.

28 **S. Arrhenius** ‘On the Influence of Carbonic Acid in the Air upon the Temperature of the Ground’, *Ibid.* **Ser. 5**, **41** (1896): 237-41. Arrhenius calculated the mean variation of temperature that would follow if atmospheric CO<sub>2</sub>

modern calculations by the state-of-the-art GCMs. Arrhenius' original concern was also what had caused the Ice Ages. His calculations thus included the effects of decrease in atmospheric CO<sub>2</sub>. But through the work of his colleague Arvid Högbom, he became aware of the increasing emissions of CO<sub>2</sub> caused by the burning of fossil fuels.<sup>29</sup> By 1904, Arrhenius argued: "the slight percentage of carbonic acid [CO<sub>2</sub>] in the atmosphere may, by the advances of industry, be changed to a noticeable degree in the course of a few centuries."<sup>30</sup>

Such an idea was not shared by many scientists at the time, for a variety of reasons. The oceanic uptake of CO<sub>2</sub> was generally thought to be large enough to offset the build-up of anthropogenic CO<sub>2</sub> in the atmosphere. The long-wave absorption band of CO<sub>2</sub> was also deemed to be narrow and to overlap that of water vapour, thereby diminishing the role of CO<sub>2</sub> in the atmospheric radiative process.<sup>31</sup> The *natural* greenhouse effect was not in question. Nor was the potential role of atmospheric CO<sub>2</sub> in controlling glacial climate change. The climatic effects of *anthropogenic* CO<sub>2</sub>, however, did not seem likely to be significant.<sup>32</sup>

Guy S. Callendar, an English steam engineer and an amateur meteorologist, was one of the few people who thought otherwise. For him, increased CO<sub>2</sub> could significantly enhance the greenhouse effect. During the late 1930s and early 1940s, Callendar published a number of papers reviving Arrhenius' theory of human-induced global warming.<sup>33</sup> In these papers, he extended Arrhenius' radiation work by using more sophisticated measurements of long-wave absorption properties of CO<sub>2</sub>. He also analysed temperature records collected from hundreds of meteorological stations over the world – albeit mainly in the Northern hemisphere. Based on these efforts, Callendar asserted that

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varied from its current mean value.

29 Arrhenius quoted Högbom's work at length in his paper. See *Ibid.* and also **E.T. Crawford** *Arrhenius: From Ionic Theory to the Greenhouse Effect* (Canton: Science History Publications, 1996)

30 **Fleming** *Historical Perspectives on Climate Change*, at 81-82. See also **S. Arrhenius** *Worlds in the Making* (New York: Harper & Brothers, 1908)

31 See **Weart** 'Global Warming, Cold War, and the Evolution of Research Plans', and references therein.

32 At the end of the 20<sup>th</sup> century, T.C. Chamberlain, an American geologist, reviewed the current hypotheses of climate change, and proposed that natural changes in the concentration of atmospheric CO<sub>2</sub> might have been related to the cycles of glacial and interglacial periods. For the work of Chamberlain, see **Fleming** 'T. C. Chamberlain, Climate Change, and Cosmogony'.

33 **G.S. Callendar** 'The Artificial Production of Carbon Dioxide and Its Influence on Temperature', *Q. J. Roy. Meteor. Soc.* **64** (1938): 223-37; 'The Composition of the Atmosphere through the Ages', *Meteorol. Mag.* **74** (1939): 33-39; 'Variations of the Amount of Carbon Dioxide in Different Air Currents', *Q. J. Roy. Meteor. Soc.* **66** (1940): 395-400; 'Infra-red Absorption by Carbon Dioxide with Special Reference to Atmospheric Radiation', *Q. J. Roy. Meteor. Soc.* **67** (1941): 263-75.

since 1890 the combustion of fossil fuels had substantially increased atmospheric CO<sub>2</sub> level, and that this could account for the observed rise in the temperature data for the last fifty years. If all other factors remained in equilibrium, he argued, human industry would further increase the mean surface temperature. Other scientists were either not convinced or rejected the argument straightaway. While Callendar's work was regarded by some as an interesting case study of atmospheric radiation,<sup>34</sup> to most scientists, CO<sub>2</sub>-induced global warming was a very remote possibility.

Callendar reiterated his claims in the late 1940s, but again without much success.<sup>35</sup> By then, temperature records were beginning to show falling average global temperatures, discouraging scientists from considering the issue of CO<sub>2</sub>-induced global warming. Some authors suggest that, in any case, in the 1940s it still seemed inconceivable that human industries could alter climate on a planetary scale.<sup>36</sup> There might also have been another reason. In the traditional cultural hierarchy of science, those scientific disciplines with high precision, control, and mathematical rigour have been specially valued, and the kind of climatology pursued by Callendar could have been lacking in those respects. As will be seen in the following chapters, it was only when the global warming hypothesis was supported by results from physico-mathematical modelling that many scientists turned to the subject.

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34 **J.R. Fleming** 'Global Warming? The Early Twentieth Century', in *Historical Perspectives on Climate Change* (Oxford: Oxford University Press, 1998): Chapter 9.

35 **G.S. Callendar** 'The Climate of the Netherlands', *Q. J. Roy. Meteor. Soc.* **73** (1947): 195-97; 'Atmospheric Radiation', *Q. J. Roy. Meteor. Soc.* **74** (1948): 81-82; 'Can Carbon Dioxide Influence Climate?' *Weather* **4** (1949): 310-14.

36 **Weart** 'Global Warming, Cold War, and the Evolution of Research Plans'; **S.R. Weart** 'The Public and Climate', in *The Discovery of Global Warming*, <http://www.aip.org/history/climate> (recorded copy at American Institute of Physics, 2001): p. Public-15 of 08/01/01 text.

## Chapter 2. Emergence of New Approaches to the Study of Climate

Well into the mid 20<sup>th</sup> century, in Britain as elsewhere, climatology – the study of climate – had been a descriptive endeavour. The climate of a locality was understood as the synthesis of weather elements affecting that region,<sup>1</sup> and the main objective of traditional climatology was considered to be no more and no less than a painstaking compilation of these elements – mainly temperature and rainfall, but also humidity, wind speed and sunshine. Monthly and annual means of each element were computed for a given area and further averaged over a specified interval of time (*e.g.*, 30 yrs). The results, usually referred to as “climate normals”,<sup>2</sup> along with the frequencies of extreme values, were then used to describe and classify regional climates, often with a view to potential applications for agriculture and commerce.<sup>3</sup> This was not an easy task. Gordon Manley, who later constructed the Central England Temperature series for the period from the mid 17<sup>th</sup> century, once wrote that amassing and processing a vast amount of observational records “demands an odd and uncommon type of enthusiasm”.<sup>4</sup> On the other hand, an exclusive emphasis on the question of “What is there?” rather than “Why is it so?” had kept the status of climatology low in the hierarchy of sciences.<sup>5</sup> As one British prominent climatologist put it, “climatology was looked upon merely as the dry-as-dust book-

1 See, for example, the definition of “climate” in **British Met Office** *The Meteorological Glossary* (London: HMSO, 1939). See also **F. Nebeker** ‘An Empirical Tradition - Climatology’, in *Calculating the Weather - Meteorology in the 20th Century* (New York: Academic Press, 1995): Chapter 2; **S.R. Weart** ‘Climatology as a Profession’, in *The Discovery of Global Warming*, <http://www.aip.org/history/climate> (recorded copy at American Institute of Physics, 2001): p. Climatology-1-12 of 08/01/01 text.

2 The notion of “climate normals” was first formalized in 1935 when the International Meteorological Organization (IMO) held the Conference of Directors in Warsaw. At the conference, the period 1901-30 was chosen as the first standard normal period in the century. See **H.H. Lamb** ‘Introduction’, in *Climate, History and the Modern World* 1<sup>st</sup> edition (London: Methuen, 1982): Chapter 1.

3 Although it is beyond the scope of this chapter, it is interesting to note that climate classification schemes were not as popular in Britain as in Germany and the US. A climatologist F.K. Hare wrote in 1951, “... Systems of classification of any natural complex make little appeal to the British temperament, which is rarely inclined to like pigeon holes or card indexes.” See **J.E. Oliver** ‘The History, Status and Future of Climatic Classification’, *Phys. Geogr.* **12** (1991): 231-51.

4 **G. Manley** ‘The Weather and Disease - Some Eighteenth Century Contributions to Observational Meteorology’, *Notes Rec. Roy. Soc.* **9** (1952): 300-07. For the Central England Temperature series, see **G. Manley** ‘The Mean Temperature of Central England, 1698-1952’, *Q. J. Roy. Meteor. Soc.* **79** (1953): 242-61; ‘Central England Temperatures - Monthly Means 1659 to 1973’, *Q. J. Roy. Meteor. Soc.* **100** (1974): 389-405.

5 **H.E. Landsberg** *Physical Climatology*, 1<sup>st</sup> edition (University Park: Pennsylvania State University Press, 1941), at xii.

keeping branch of meteorology”.<sup>6</sup>

Climatologists at the time were not expected to have acquired the knowledge and skills of physics and applied mathematics. So it was no surprise that most of them had little interest in a thorough examination of various processes governing the earth’s climate, including solar radiation, land/water distribution, ocean currents and large-scale atmospheric circulation. The conventional descriptive and regional approach in climatology was also related to the then prevailing assumption that climate could be treated as if it were constant.<sup>7</sup> It was presumed that, although weather elements varied with time, their ranges and mean values would deviate only slightly from “climate normals” on the decadal or even centenary time-scale. The observational studies based upon such an assumption worked for practical purposes, or at least they were perceived to do so.<sup>8</sup> There was thus no pressing need for climatologists to adopt a new perspective. Meteorologists, too, for their part, accepted the static conception of climate. While they were better equipped to address theoretical questions about the general behaviour of the atmosphere than traditional climatologists, their discussions tended to be limited to the development of short-term regional weather patterns. The rare exceptions that touched on the physics of climate, albeit qualitatively, were those studies of glacial-interglacial climate changes by geologists, palaeobotanists and astrophysicists. And only a very few climatologists and meteorologists enthusiastically participated in this type of research.<sup>9</sup>

Within a few decades, the scope and method of climatology were considerably transformed. The observational studies of climate continued to be carried out, but with a non-static view of climate – that is, an understanding of climate as the result of the

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6 **H.H. Lamb** 'Our Changing Climate, Past and Present', *Weather* **14** (1959): 299-318.

7 Ibid.

8 **H.H. Lamb** *Climate, History and the Modern World*, 1<sup>st</sup> edition (London: Methuen, 1982)

9 In Britain, the most notable example was C.E.P. Brooks at the Met Office, who continually examined a range of different studies of climate and its change. See **C.E.P. Brooks** *Climate Through the Ages - A Study of the Climatic Factors and Their Variations* (London: E. Benn, 1926); 'Geological and Historical Aspects of Climatic Change', in *Compendium of Meteorology*, T.F. Malone (ed) (Boston: American Meteorological Society, 1951): 1004-18; 'Present Position of Theories of Climatic Change', *Meteorol. Mag.* **84** (1955): 204-06. Brooks held a first and MS degrees in geology and a DSc degree in meteorology and climatology. See **A.H.R. Goldie** 'Obituary - C. E. P. Brooks', *Q. J. Roy. Meteor. Soc.* **84** (1958): 202-03. Another example was G.C. Simpson, who was director of the Meteorological Office from 1920 to 1938. See **G.C. Simpson** 'World Climate during the Quaternary Period', *Q. J. Roy. Meteor. Soc.* **60** (1934): 425-71; 'Probable Causes of Change in Climate and Their Limitations', *Proceedings of the Linnaean Society of London* **152** (1940): 190-219; 'Further Studies in World Climate', *Q. J. Roy. Meteor. Soc.* **83** (1957): 459-85. It must be pointed out, however, that the multidisciplinary identity of climatology often makes it difficult to distinguish climatologists from scientists in the adjacent disciplines.



workings of large-scale atmospheric circulation. The physical principles began to be applied quantitatively to study the global energy balance of the earth's surface and the atmosphere. There also emerged a new distinct class of researchers, who dealt primarily with numerical modelling of climate and its change. Built upon the fundamental equations of hydrodynamics and thermodynamics, complex numerical models were constructed to simulate the general circulation of the atmosphere. These new approaches to the study of climate laid the basis for the emergence of the modern science of global climate change. Among these, the numerical modelling approach, with both physical and dynamical processes controlling climate as a major focus of inquiry, was most favoured by mainstream meteorologists. By the mid to late 1970s, it came to gain intellectual hegemony within the field of climate research. This line of approach, like the other two, began to take shape during the 1950s. Some of its origins, however, could be traced back to advances in meteorology and climatology in the early 20<sup>th</sup> century.

### Development of Scientific Meteorology

The idea of applying the laws of physics to large-scale atmospheric motions had already been cultivated in the late 19<sup>th</sup> century by a number of scientists, such as Hermann Helmholtz in Germany, James Thompson in England, and William Ferrel in the US.<sup>10</sup> Yet, at the beginning of the early 20<sup>th</sup> century in Britain, meteorology itself was considered to lack a solid scientific basis, and few scientists with a physics or mathematics background turned their attention to the subject. The Royal and Scottish Meteorological Societies were essentially associations of amateur meteorologists. So was the British Rainfall Organization (BRO). Nor were there any meteorology units or departments in British universities. The Met Office had only a tiny number of staff with formal scientific training. Even in 1910, there were just five staff members who had a science degree, including Napier Shaw, its director and a former Cambridge physicist.<sup>11</sup>

Gradually, the situation began to change. In 1905, Manchester University's physics

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10 For the 19<sup>th</sup>-century history of dynamical meteorology, see **A.K. Khrgian** *Meteorology: A Historical Survey*, 2<sup>nd</sup> edition (Jerusalem: Israel Program for Scientific Translations, 1970); **F. Nebeker** *Calculating the Weather - Meteorology in the 20th Century* (New York: Academic Press, 1995)

11 **N. Shaw** 'The March of Meteorology - Random Recollections', *Q. J. Roy. Meteor. Soc.* **60** (1934): 101-20; **D. Brunt** 'Centenary of the Royal Meteorological Society', *Nature* **165** (1950): 463-66.

department created the first lectureship in meteorology in Britain. A year later, the Cavendish Laboratory at Cambridge established a readership in meteorology. These posts were temporary and were founded owing to the efforts of Arthur Schuster, a physics professor at Manchester, rather than to an acceptance of meteorology as a physical science discipline.<sup>12</sup> They nevertheless signalled a growing application of physical and mathematical approaches to atmospheric problems. In the Met Office, Shaw not only worked hard to attract young physics/mathematics graduates to the Office<sup>13</sup> but also accelerated such a shift in meteorology through his own scientific research. In a series of papers, for example, he laid down basic thermodynamic principles for the construction and interpretation of air parcel trajectories.<sup>14</sup>

A notable step in this development was the ground-breaking work of Vilhelm Bjerknes, a Norwegian mathematical physicist. He argued in 1904 that the behaviour of the atmosphere, given its initial state, should in principle be predicted by the governing thermodynamic and hydrodynamic equations derived from known physical laws,<sup>15</sup> opening the way towards “dynamical meteorology” and numerical weather prediction. Bjerknes’ proposal was put to the test and further refined by Lewis Fry Richardson at the Met Office. Around 1911, Richardson tentatively began to employ a method of finite differences to the approximate solutions for differential equations describing the atmosphere.<sup>16</sup> His numerical methodology was well received and regarded as an

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12 Schuster hired one of his disciples, Simpson, as a meteorology lecturer in his department. Unfortunately, a year later, Simpson left Manchester to join the Indian branch of the Met Office. Schuster then persuaded Cambridge, with his endowment, to open a readership in 1906. See **G.C. Simpson** 'Sir Arthur Schuster', *Obit. Notices Fellows Roy. Soc.* **1** (1932-35): 409-23; **E. Gold** 'Sir George Clarke Simpson', *Biogr. Mem. Fellows R. Soc.* **11** (1965): 157-75. The first reader was E. Gold, a Cambridge wrangler and Natural Sciences Tripos. At Cambridge, Gold carried out a theoretical analysis of the atmospheric motion. He also provided a theoretical explanation for the effect of radiation on the upper atmosphere. When Gold joined the Met Office in 1910, G. I. Taylor, also a wrangler and Natural Sciences Tripos, became the second Schuster reader and pioneered the mathematical investigation of atmospheric turbulences. See also **R.C. Sutcliffe and A.C. Best** 'Ernest Gold', *Biogr. Mem. Fellows R. Soc.* **23** (1977): 115-31; **G.K. Batchelor** 'Sir Geoffrey Ingram Taylor', *Biogr. Mem. Fellows R. Soc.* **22** (1976): 565-633. Meanwhile, Simpson continued his work on atmospheric electricity that he began in Manchester, until he succeeded Shaw as director of the Met Office in 1920.

13 Shaw had also held a part-time readership in meteorology at London University since 1907. See **E. Gold** 'Sir William Napier Shaw', *Obit. Notices Fellows Roy. Soc.* **5** (1945-48): 203-30.

14 See, e.g., **W.N. Shaw and R.G.K. Lempfert** *The Life History of Surface Air Currents*, Meteorological Office Memoirs, Vol. 14 (London: HMSO, 1906)

15 **Nebeker** *Calculating the Weather*. In fact, the calculation of atmospheric changes using dynamical methods was first tried by Felix Exner at Vienna in 1908. See **G.P. Cressman** 'The Origin and Rise of Numerical Weather Prediction', in *Historical Essays on Meteorology, 1919-1995*, J.R. Fleming (ed) (Boston: American Meteorological Society, 1996): 21-39.

16 Richardson was introduced by Shaw to the work of Bjerknes when he was appointed as Superintendent of the Met Office's Eskdalemuir Observatory. See **E. Gold** 'Lewis Fry Richardson', *Obit. Notices Fellows Roy. Soc.* **9** (1954): 217-35.

important contribution to modern meteorology. Unfortunately for Richardson, the absence of adequate calculating aids and upper-air data led to disastrous computation results,<sup>17</sup> and this dissuaded others from pursuing the same line of attack on the problems of meteorology.

Meanwhile, World War I and the subsequent growth of aviation, both civil and military, resulted in increasing needs for weather services. The Met Office rapidly expanded, and in 1919, came under the Air Ministry, uniting the meteorological divisions of the Royal Air Force, the Navy and the Army.<sup>18</sup> However, as Richardson's experiment revealed, new theoretical developments were of no use in forecasting – the most important part of weather services. It was again Bjerknes, now at Bergen, who came up with a compromise solution. He and his students devised the frontal and air-mass analysis that was practical and qualitative, though based upon physical concepts.<sup>19</sup> At first, their synoptic method, as it came to be called, encountered some resistance from British forecasters who had been using more subjective, experience-based techniques. Eventually, the Met Office's senior scientists saw its relevance. In 1925, the Office invited Jacob Bjerknes, Vilhelm's son, and Tor Bergeron, a Swedish meteorologist at Bergen, to train its forecasters.<sup>20</sup> Within a decade, the Bergen method came to be widely adopted as a basis for practical weather services in Britain and elsewhere.<sup>21</sup>

These changes by no means ran counter to the transformation of meteorology into a physical science. Synoptic method was, to an important degree, compatible with a newly developed physical understanding of the atmosphere. At the same time, a rising interest in

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17 He carried out calculations manually while he was serving as an ambulance driver in France during 1916-18. See *Ibid.* The results were later published in his book, **L.F. Richardson** *Weather Prediction by Numerical Process* (London: Cambridge University Press, 1922)

18 The reorganization of the Met Office was not welcomed by all meteorologists. Richardson, a Quaker, resigned from the Office as his pacifist principles clashed with working for the Air Ministry. Others were more worried about the autonomy of the Met Office. See **Gold** 'Lewis Fry Richardson'; **O.G. Sutton** 'Sir David Brunt', *Biogr. Mem. Fellows R. Soc.* **11** (1965): 41-52. See also **R.M. Friedman** *Appropriating the Weather - Vilhelm Bjerknes and the Construction of a Modern Meteorology* (Ithaca: Cornell University Press, 1989)

19 This method analyzed the structure and development of a region in the atmosphere in terms of fronts and air-masses. *Front* generally refers to the interface or transition zone between two air masses of different density. *Air-mass* refers to an extensive body of the atmosphere in which physical properties, particularly temperature and humidity, exhibit only small and continuous differences in the horizontal. It is noteworthy that, in the 1910s, Napier Shaw also sought to connect weather forecasting with physical processes of the atmosphere and with air mass trajectories, although it was Bjerknes who realized his goal. For Shaw's work on forecasting, see **W.N. Shaw** *Forecasting Weather* (London: Constable & Company Ltd., 1911)

20 **O.M. Ashford** 'Development of Weather Forecasting in Britain, 1900-1940 - The Vision of L. F. Richardson', *Weather* **47** (1992): 394-403.

21 **J. Namias** 'The History of Polar Front and Air Mass Concepts in the United States - An Eyewitness Account', *B. Am. Meteorol. Soc.* **64** (1983): 734-55.



the practical value of meteorology brought many physics/mathematics graduates into the Met Office, who would otherwise have had little interest in it. The economic depression of the late 1920s further encouraged such a career move.<sup>22</sup> These new meteorologists were naturally more inclined towards physical and mathematical approaches, and their entry reinforced the ongoing transition in the field. A growth of aviation also helped form an institutional base for meteorology in academia. Napier Shaw, upon retiring from the Met Office in 1920, took up a part-time professorial chair at Imperial College in London and inaugurated Britain's first academic unit in meteorology as the sub-department of the aeronautics department.<sup>23</sup> Later, in 1934, the unit became a graduate department with David Brunt as a full-time professor and recruited primarily those holding a first degree in physics or mathematics.<sup>24</sup> In Oxford, Gordon M.B. Dobson, who had been director of the Royal Aircraft Establishment's Experimental Department, joined the Clarendon Laboratory in 1920 as a lecturer in meteorology and initiated the study of the upper atmosphere, ozone measurements in particular.<sup>25</sup>

As (since 1935 Sir) George Simpson, Shaw's successor as director of the Met Office, reported in 1939, Britain still lagged behind other countries in the academic teaching of modern meteorology.<sup>26</sup> In the Met Office, the study of weather continued to be dominated by the use of weather maps. By the mid to late 1930s, though, the portrayal of meteorology as a branch of physical sciences was no longer uncommon.

22 **R.C. Sutcliffe** *Interview by J.M.C. Burton* (Royal Meteorological Society Interview: 1983); **H. Taba** 'The Bulletin Interviews - J.S. Sawyer', *WMO Bulletin* **46** (1997): 104-15.

23 Shaw was succeeded in 1924 by Gilbert Walker, the head of the Met Office's Indian Department. Walker, a senior wrangler in 1889, had been a lecturer in mathematics at Cambridge until he joined the Met Office in 1903. He retired from Imperial in 1934. See **Gold** 'Sir William Napier Shaw'; **G.I. Taylor** 'Sir Gilbert Thomas Walker', *Biogr. Mem. Fellows R. Soc.* **8** (1962): 167-74.

24 Brunt, a Cambridge wrangler at the Met Office, was already assisting Shaw as a visiting lecturer at Imperial during the 1920s. He later wrote a seminal text, **D. Brunt** *Physical and Dynamical Meteorology*, 1<sup>st</sup> edition (Cambridge: Cambridge University Press, 1934). See also **Sutton** 'Sir David Brunt'. At the time in Imperial, Sydney Chapman, another of Schuster's former students and a professor of mathematics, was also interested in meteorology though his main interests were geomagnetism and ionospheric physics. **T.G. Cowling** 'Sydney Chapman', *Biogr. Mem. Fellows R. Soc.* **17** (1971): 53-89.

25 See **J.T. Houghton and C.D. Walshaw** 'Gordon Miller Bourne Dobson', *Biogr. Mem. Fellows R. Soc.* **23** (1977): 41-57; **G.M.B. Dobson** 'Forty Years' Research on Atmospheric Ozone at Oxford - A History', *Appl. Optics* **7** (1968): 387-405. In Cambridge, Taylor resumed his academic career after World War I, now as a lecturer in mathematics. He worked on the application of turbulent flow to oceanography for some time, but did not continue to pursue his research career in meteorology and oceanography. Since 1919, E.V. Appleton at Cavendish had also been carrying out research in ionospheric physics. But Appleton was much less active in communicating with the meteorological community than Taylor. See **Batchelor** 'Sir Geoffrey Ingram Taylor'; **J.A. Ratcliffe** 'Sir Edward Victor Appleton', *Biogr. Mem. Fellows R. Soc.* **12** (1966): 1-21.

26 **G.C. Simpson** *Notes on Research and Training in the Meteorological Bureau and University in Response to the Letter from the Minister for the Interior dated 18 January, 1939* (March 11, 1939), quoted from **J. Gardner** 'Stormy Weather: A History of Research in the Bureau of Meteorology', in *Federation and Meteorology*, H. Morgan (ed) (Melbourne: Australian Science and Technology Heritage Centre, 2001): 802.

## Descriptive Climatology and Beyond

The introduction of physical and mathematical approaches was less apparent in climatology. In retrospect, an improved understanding of atmospheric processes could have equally stimulated climatology. This link was not so clear at that time. As noted above, for most meteorologists and climatologists, the study of climate was yet a rather unscientific, book-keeping branch of meteorology.

Book-keeping activities were of course important in themselves. Reliable climate records had long been thought to be useful for agriculture and commerce. The Met Office had specific branches for the collection, analysis and publication of these data over the British Isles. One problem at the beginning of the 20<sup>th</sup> century was that most of the observations were being conducted by amateurs, who then reported their results to the Royal and Scottish Meteorological Societies and BRO.<sup>27</sup> The unification of climatological services was sought in order to avoid wasteful overlap and secure regular observations to minimum standards. After lengthy negotiations, the climatological sections of the two societies were transferred to the Met Office in 1914. The BRO became part of the Met Office in 1919 when the restructuring of the Office took place.<sup>28</sup> But these observational studies of climate were perceived as a “half-hearted business” within the Met Office, and scientific staffs were unwilling to devote their energies to them.<sup>29</sup>

The analysis of climatic data was, in fact, non-trivial. The sheer size of climatic data often required complex methods in order to obtain better representations of salient features of the observed climate. This was even more the case for those working on long-range or seasonal forecasting. Unlike short-range forecasters, they had to look at the wider weather picture over a period of time, not day-to-day details, and to search for relationship between various parameters in the data sets.<sup>30</sup> This inevitably demanded the use of

27 In the 1910s, the BRO alone had more than 5,000 voluntary observers around the country. See **J.M.C. Burton** 'Pen portraits of Presidents - Sir Napier Shaw', *Weather* **50** (1995): 89-93.

28 See **Gold** 'Sir William Napier Shaw'.

29 **E. Gold** 'Incidents in the "March," 1906-1914', *Q. J. Roy. Meteor. Soc.* **60** (1934): 121-25. It may be noted that, during the first half of the 20<sup>th</sup> century in Britain, observational studies of climate were not popular even in geography departments, but for somewhat different reasons. Atkins suggested that a strong reaction to climatic determinism might have discouraged geographers from working on climatology. See **B.W. Atkinson** 'Climate', in *Geography, Yesterday and Tomorrow*, E.H. Brown (ed) (Oxford: Oxford University Press, 1980): 114-29. On the climatic determinism of Ellsworth Huntington, see **J.R. Fleming** 'The Climate Determinism of Ellsworth Huntington', in *Historical Perspectives on Climate Change* (Oxford: Oxford University Press, 1998): Chapter 8.

30 For a brief history of synoptic climatology, see chapters 1 and 2 of **R.G. Barry and A.H. Perry** *Synoptic Climatology - Methods and Applications* (London: Methuen, 1973)

rigorous statistical techniques. For example, in the 1910s and 1920s, Gilbert Walker, director of the Indian Meteorological Department, applied the methods of correlation analysis to climatic data in developing a seasonal forecast scheme for the monsoon and for the winter rainfall in northern India.<sup>31</sup>

The use of statistical methods in climatological work was respected by meteorologists,<sup>32</sup> but the scientific status of climatology, and that of long-range forecasting, remained in question, since the underlying physical processes were not yet being thoroughly taken into account. The synoptic method was again put forward as a solution. In 1929, Tor Bergeron argued for the application of frontal and air-mass methods to climatology. In outlining what he referred to as “dynamic climatology”, he claimed that climate should be treated as the totality of atmospheric processes rather than seen in terms of the unrelated distribution of individual meteorological elements.<sup>33</sup> The statistical treatment of stable weather types – air-mass and fronts – could then serve as a tool towards a physical explanation of the mean atmospheric circulation and its variation.

Other developments, which may not have been originally intended for climatologists, also pointed towards a new direction. In 1917, William Dines at the Met Office calculated the heat balance of the atmosphere from the downward long-wave observations, which he obtained using his instruments, and modified the radiation budget determined by US astrophysicists Charles Abbot and Frederick Fowle.<sup>34</sup> In the late 1920s, Simpson looked into the more recent infrared observations, and provided estimates for both hemispheres and geographical distribution of the components in the earth’s radiation budget.<sup>35</sup> These investigations led him to consider the causes of Ice Ages and to formulate his solar radiation theory of glaciations. Simpson was not alone in linking radiation studies to the

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31 This study enabled him to identify, for the first time, the Southern Oscillation (SO) – an irregular pulsation of atmospheric pressure between the Pacific and the Indian Oceans. As mentioned above, Walker was a Cambridge lecturer in mathematics before he joined the Met Office. In 1924, he became a professor of meteorology at Imperial College. See **K.K. Kumar et al.** ‘Seasonal Forecasting of Indian Summer Monsoon Rainfall - A Review’, *Weather* **50** (1995): 449-67. See also **Taylor** ‘Sir Gilbert Thomas Walker’.

32 For the Met Office’s contribution in this area, see **C.E.P. Brooks and N. Carruthers** *Statistical Methods in Climatology* (London: HMSO, 1944)

33 **A. Court** ‘Climatology: Complex, Dynamic, and Synoptic’, *Ann. Assoc. Am. Geogr.* **47** (1957): 125-36.

34 **G.E. Hunt et al.** ‘A History of Presatellite Investigations of the Earth’s Radiation Budget’, *Rev. Geophys.* **24** (1986): 351-56. The earth’s weather and climate are controlled by the balance between the absorbed solar energy and the thermal energy emitted to space. This critical energy balance is called the earth’s radiation budget.

35 **G.C. Simpson** ‘Some Studies in Terrestrial Radiation’, *Memoirs of the Royal Meteorological Society* **2** (1928): 69-95; ‘Further Studies in Terrestrial Radiation’, *Memoirs of the Royal Meteorological Society* **3** (1928): 1-26; ‘The Distribution of Terrestrial Radiation’, *Memoirs of the Royal Meteorological Society* **3** (1929): 53-78.

topic of climate and its change. A Serbian astrophysicist, Milutin Milankovitch, suggested the astronomical theory of climatic change in 1920, based on his insolation calculations.<sup>36</sup> In the late 1930s, Guy S. Callendar, an amateur climatologist, began to work on the absorption of radiation by the atmosphere and revived the CO<sub>2</sub> theory of climate change.<sup>37</sup> In the US, William J. Humphreys of the Weather Bureau tackled similar issues,<sup>38</sup> while Chaim L. Pekeris at MIT and Walter M. Elsasser at Harvard discussed the heat balance in the atmosphere.<sup>39</sup>

But studies such as these did not have an immediate impact on the way climatology was practised in Britain. Bergeron's "dynamic climatology" was qualitative and intuitive, comparable to what we now understand as "synoptic climatology".<sup>40</sup> A more mathematically oriented outline of dynamical climatology, closer to the current use of the term, was proposed later in 1932 by Theodore Hesselberg, director of the Norwegian Meteorological Institute.<sup>41</sup> Still, Bergeron's scheme involved comprehensive knowledge of hydrodynamics and thermodynamics, which traditional climatologists found quite difficult to grasp. Thus, although his pioneering work appealed to some climatologists,<sup>42</sup> it was not generally taken up. The studies of atmospheric radiation were received perhaps more readily by the broad meteorological community. The works of Simpson, Callendar and Humphreys, for instance, were regarded highly by meteorologists as valuable contributions to the physical understanding of atmospheric processes. Yet their discussion of climate and its change found relatively little resonance, and did not give rise to the development of physics-based climatology in Britain.

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36 **J.E. Kutzbach** 'Steps in the Evolution of Climatology - From Descriptive to Analytic', in *Historical Essays on Meteorology, 1919-1995*, J.R. Fleming (ed) (Boston: American Meteorological Society, 1996): 353-77.

37 **G.S. Callendar** 'The Artificial Production of Carbon Dioxide and Its Influence on Temperature', *Q. J. Roy. Meteor. Soc.* **64** (1938): 223-37.

38 **W.J. Humphreys** *Physics of the Air*, 2<sup>nd</sup> edition (New York: McGraw-Hill, 1929)

39 **C.L. Pekeris** *The Development and Present Status of the Theory of the Heat Balance in the Atmosphere* (Cambridge: MIT Department of Meteorology, 1934); **W.M. Elsasser** *Heat Transfer by Infrared Radiation in the Atmosphere*, Harvard Meteorological Studies, No. 6 (Cambridge: Harvard University Press, 1942)

40 See **Court** 'Climatology: Complex, Dynamic, and Synoptic'.

41 Hesselberg was one of Vilhelm Bjerknes' early students and worked for him as an assistant (with the funding of Carnegie Institution) before he joined the Norwegian Meteorological Institute. See **Friedman** *Appropriating the Weather*: Chapters 3 and 4.

42 See **Lamb** *Climate, History and the Modern World*.

## World War II, Meteorology and Climatology

The wartime importance of meteorology had been fully illustrated by World War I. Given the larger scale of the war and the enhanced performance capabilities of aircraft, the role of meteorology was even greater in World War II. Due to the forecasting requirements of the fighting forces, the number of meteorologists rose rapidly. By the time the war ended in 1945, the staff of the Met Office had increased to over 6,000, almost ten-fold as compared with the pre-war level.<sup>43</sup> The membership of the Royal Meteorological Society also doubled, from 879 in 1940 to 1714 in 1947.<sup>44</sup>

The rapid expansion in weather services during the war, however, did not directly translate into strong support for meteorological research in Britain. The Met Office had done virtually no formal organized research before World War II, except for the study of atmospheric diffusion at the Chemical Warfare Experimental Station, Porton. The culture of the Office was such that research activities had been left largely to the commitment of individual staff.<sup>45</sup> N.K. (later Sir Nelson) Johnson, who succeeded Simpson as director in 1938, had previously been a research leader at Porton and was eager to build strong research within the Office. He appointed an assistant director to supervise investigational work and planned to set up a research section. The plan was bolstered in 1941 by the establishment of the Air Ministry's Meteorological Research Committee,<sup>46</sup> only to be postponed because of the immense operational necessity of the war. Apart from a small upper-air analysis group at Dunstable, there had been little opportunity for the Office staff to carry out research.<sup>47</sup> David Brunt's effort to expand his meteorology department at Imperial was also interrupted as he was deeply engaged in government advisory work and in training military meteorologists.<sup>48</sup>

There were some improvements in forecasting techniques, but they were based on the same synoptic method that had been used since the 1930s. As a senior forecaster C.K.M.

43 **G. Sutton** 'The Meteorological Office, 1855-1955', *Nature* **175** (1955): 963-65.

44 **M. Walker** 'The Royal Meteorological Society As Seen Through Its Membership', *Weather* **55** (2000): 104-08.

45 Simpson, director until 1938, in particular believed that good scientists should be able to find their own time and projects for research in addition to their official duties. He himself carried out his research on terrestrial radiation in his spare time. See **Gold** 'Sir George Clarke Simpson'.

46 **F.J. Scrase** 'The History of the Meteorological Research Committee', *Meteorol. Mag.* **91** (1962): 310-14; **Sutcliffe** *Interview by J.M.C. Burton*.

47 **R.A.S. Ratcliffe** 'Weather Forecasting in Britain, 1939-80', *Weather* **48** (1993): 299-304.

48 **Sutton** 'Sir David Brunt'.



Douglas wrote in 1952, these changes were “innocent of dynamic theory”, and had “little permanent effect” on the future development of meteorology.<sup>49</sup> Far more important was the vast extension of observing networks. At the end of the war, there were more than 550 manned meteorological stations in the British Isles alone, most of which were located at the Royal Air Force and the US Air Force airfields.<sup>50</sup> High-flying reconnaissance and bombing missions brought about an even greater increase in upper-air observations. In pre-war Britain, upper-air observations were sporadic and the data collected by conventional means were unreliable. By 1942, the Met Office was routinely collecting upper-air data over Britain and much of German-occupied Europe by using a new instrument, radiosondes.<sup>51</sup> For the first time, weather forecasters were able to produce upper-air synoptic maps, in addition to surface weather maps, exhibiting the three-dimensional motion of the atmosphere. Improved observations of this kind provided the basis for new developments in weather forecasting and for more advanced studies of atmospheric circulation in the post war era.

The wartime impact on climatology was no less significant. Strategic planning for military operations necessitated information about how weather elements combined over large areas. Extracting the correlated information from historical climatic records by usual statistical methods was time-consuming, and often proved impossible.<sup>52</sup> To solve this problem, C.S. Durst and C.E.P. Brooks at the Met Office’s Investigations Division applied synoptic ideas to climatology and set out an analysis in which the conditions of local or regional climates could be inferred by relating weather elements to atmospheric circulation processes.<sup>53</sup> This “synoptic climatology” was developed as an empirical method of meeting operational needs and was regionally focused, but it resembled Bergeron’s “dynamic climatology” in that the physical concepts and variables relating to

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49 Douglas even went as far as to say, “the word ‘theory’ is unsuitable for anything in synoptic meteorology. It is really a technique based on simplified models of atmospheric structure and movement, and its successes have been based on its empirical rather than its theoretical aspects”. **C.K.M. Douglas** ‘The Evolution of 20<sup>th</sup> Century Forecasting in the British Isles’, *Q. J. Roy. Meteor. Soc.* **78** (1952): 1-21.

50 **G. Cowling** ‘A Forecaster Recalls - 1946’, *Weather* **51** (1996): 151-56.

51 **R.A.S. Ratcliffe** ‘Weather Forecasting in Britain, 1939-80’, *Ibid.* **48** (1993): 299-304.

52 **F.K. Hare** ‘Dynamic and Synoptic Climatology’, *Ann. Assoc. Am. Geogr.* **45** (1955): 152-62.

53 *Ibid.*; **H. Taba** ‘The Bulletin Interviews - F.K. Hare’, *WMO Bulletin* **39** (1990): 3-16; **C.S. Durst** ‘Climate - the Synthesis of Weather’, in *Compendium of Meteorology*, T.F. Malone (ed) (Boston: American Meteorological Society, 1951): 967-75. Durst graduated in mathematics from Cambridge. He then served with the Royal Engineers for several years before joining the Met Office in 1920. See **Anon.** ‘Obituary - Charles Sumner Durst’, *Q. J. Roy. Meteor. Soc.* **88** (1962): 205-06.

atmospheric dynamics were utilized.<sup>54</sup> The wide use of this approach produced a new class of climatologists, who trained themselves in the physics of atmospheric motions and then disseminated a new conception of climate – *i.e.*, climate was viewed no longer as a mere statistical collection of weather conditions but as the result of the workings of large-scale atmospheric circulations.

Military interest in medium- and long-range weather forecasting also directed meteorologists' attention to what was essentially climatological work. In 1942, under the urgent pressure of wartime requirements, the Met Office carried out an experiment to predict the pressure field over periods from several days to a few months.<sup>55</sup> Harmonic analysis and symmetry points in time-series were employed to identify frequency components in climatic records. The experiment failed and was abandoned after 18 months. At about the same time, with the help of Gilbert Walker as a consultant, another experiment was carried out into the association between Arctic sea-ice and European weather.<sup>56</sup> Again, it turned out to be a failure. These trials deepened meteorologists' scepticism about the desirability of long-range forecasting based on empirical-statistical techniques. Just after the war, J.M. Craddock recalled, "some of the senior people in the Office took the view that climatic forecasting not only was not possible, but also that it was none of their business to attempt it".<sup>57</sup> World War II nonetheless highlighted the potential practical importance of extended forecasting, thereby increasing contacts between meteorologists and climatologists.

### Early Physico-Dynamical Studies of General Circulation

Military interests in meteorology and climatology waned slightly after the war, but were instantly resumed with the emergence of the Cold War. The Royal Air Force and the US

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54 Its American counterpart was developed by W.C. Jacobs of the Air Weather Services during the war. Interestingly, the British synoptic climatology approach was somewhat more narrative and less numerical. See **W.C. Jacobs** *Wartime Developments in Applied Climatology*, Meteorological Monographs, Vol. 1, No. 1 (Boston: American Meteorological Society, 1947); **Court** 'Climatology: Complex, Dynamic, and Synoptic'. For early discussions over the relationship between synoptic and dynamical climatology, see also **Hare** 'Dynamic and Synoptic Climatology'; **F.K. Hare** 'The Dynamic Aspects of Climatology', *Geogr. Ann. A.* **39** (1957): 87-104.

55 **J.S. Sawyer** 'Research in Synoptic and Dynamical Meteorology and in Climatology - 1941 to 1962', *Meteorol. Mag.* **91** (1962): 327-35.

56 *Ibid.*

57 **J.M. Craddock** 'Long-range Weather Forecasting from 1947 Onwards', *Weather* **50** (1995): 403-07.

Air Force were increasingly deployed again during the Korean War and the Suez Crisis.<sup>58</sup> Civil aviation also grew exponentially. The demands on meteorological services persisted, especially for upper wind forecasts. Since 1946, more accurate upper-air instruments, rawinsondes, began to be launched in both Britain and the US, widening the existing observing networks.<sup>59</sup> In the meantime, a series of atmospheric nuclear weapons tests had been conducted throughout the post-war years. When the first British test took place on the north west coast of Australia in 1952, radioactive hailstones fell nearly 3,000 km away from the test site. The danger posed by radioactive fallout from nuclear tests emerged as a matter of public concern.<sup>60</sup> Together with increased upper-air data, these events prompted meteorologists and climatologists to investigate high-level atmospheric general circulations.<sup>61</sup>

More sophisticated studies of the general circulation had already been commenced in the late 1930s by Carl-Gustav Rossby at MIT.<sup>62</sup> Rossby's work was more faithful to Bjerknes' original idea than Bergeron's. Building upon the early work of Bjerknes, he devised a simplified form of barotropic equations for certain large-scale motions of the atmosphere and showed that these equations could be solvable and could simulate observations.<sup>63</sup> This would have huge implications for both meteorology and climatology,

58 **S.J. Caughey and P.W. Davies** 'Defence Services Branch 50<sup>th</sup> Anniversary, Part I. - Historical Aspects', *Meteorol. Mag.* **118** (1989): 161-68.

59 A rawinsonde is an acronym for ra(dar) + win(d) + (radio)sonde – radiosonde used to observe the velocity and direction of upper-air winds and tracked by a radio direction-finding instrument or radar.

60 See **M. Veldman** *Fantasy, the Bomb, and the Greening of Britain - Romantic Protest, 1945-1980* (Cambridge: Cambridge University Press, 1994); **G. Sutton** 'Thermonuclear Explosions and the Weather', *Nature* **175** (1955): 319-21. It is noteworthy that public anxieties over nuclear fallout prompted the US Federal support for <sup>14</sup>C tracer studies, mainly through the Atomic Energy Commission (AEC) and the Office of Naval Research (ONR). The funding of AEC and ONR allowed Revelle and Suess to work on ocean/atmosphere carbon exchange and eventually on the greenhouse effect. See Chapters 3 and 4. See also **D.G. Victor and W.C. Clark** *The Greenhouse Effect in the US - A History of the Science Up To 1985*, Contribution I-2 on the project on Social Learning in the Management of Global Environmental Risks (Cambridge: Kennedy School of Government, Harvard University, 1991); **D.M. Hart and D.G. Victor** 'Scientific Elites and the Making of US Policy for Climate Change Research, 1957-74', *Soc. Stud. Sci.* **23** (1993): 643-80; **S.R. Weart** 'Roger Revelle's Discovery', in *The Discovery of Global Warming*, <http://www.aip.org/history/climate> (recorded copy at American Institute of Physics, 2001): p. Revelle-3~5 of 08/01/01 text.

61 **H.H. Lamb** *Interview by R.A.S. Ratcliffe* (Royal Meteorological Society Interview: April 25, 1985)

62 During the late 1910s, Rossby worked for two years at Vilhelm Bjerknes' institute at Bergen. He then studied mathematical physics at the University of Stockholm and in 1925 moved to the US. In 1928, he created the first US department of meteorology at MIT and became its first chair. He was also responsible for setting up the meteorology department at Chicago in 1940. In 1947, Rossby went back to Sweden to head the Meteorological Institute of Stockholm University. See **Friedman** *Appropriating the Weather*; **W.A. Koelsch** 'From Geo- to Physical Science - Meteorology and the American University, 1919-1945', in *Historical Essays on Meteorology, 1919-1995*, J.R. Fleming (ed) (Boston: American Meteorological Society, 1996): 511-40.

63 **A. Wiin-Nielsen** 'The Birth of Numerical Weather Prediction', *Tellus* **43** (1991): 36-52; **Cressman** 'The Origin and Rise of NWP'. Barotropic models assumed that the temperature along a constant pressure surface would not vary and atmospheric motions would not change with height, yielding a prediction at only one level (typically 500 hPa). This assumption gave a simple but poor representation of the atmosphere. Nonetheless, it



since large-scale features of weather and climate could now be deduced in practice from the inherent dynamics of the atmosphere without reference to the variations in the earth's surface. However, even though his barotropic equations were derived for the analytic study – as opposed to a quantitative prediction – of atmospheric motions, they required immense calculations. The lack of computing power remained problematic.

The advent of electronic computing took some time. As upper-air data accumulated, some meteorologists adopted a different approach. In the late 1940s, Charles H.B. Priestley in Australia and Jacob Bjerknes and Victor Starr in the US independently proposed to study empirically the details of the three-dimensional atmospheric circulation, taking advantage of the newly available regular upper-air wind and temperature data.<sup>64</sup> This suggestion was put into practice most extensively by Starr's General Circulation Project at MIT. Using the enhanced upper-wind and radiosonde observations over the Northern Hemisphere, they documented the flow of heat, moisture and angular momentum that maintained the general circulation – *i.e.*, the actual dynamics of large-scale atmospheric processes.<sup>65</sup> These studies were observational in nature, but were driven as much by theoretical considerations as by new observations – they belonged to what might be called “the observational branch of dynamical climatology”.<sup>66</sup>

These studies were widely known across the Atlantic.<sup>67</sup> In Britain, though, observational studies of the general circulation were not pursued until the mid to late 1950s. The general circulation of the atmosphere was without a doubt considered as an important research problem. Nelson Johnson, after reviewing the history of meteorology over the last hundred years, asserted in 1950 that for future development, “the most important and fundamental problem is the study of the world circulation as a whole”.<sup>68</sup> But the Met Office was above all a government service agency. Most of the resources

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was believed to provide a considerable computational advantage.

64 **C.H.B. Priestley** 'Heat Transport and Zonal Stress between Latitudes', *Q. J. Roy. Meteor. Soc.* **75** (1949): 28-40; **V.P. Starr** *Investigation of the General Circulation of the Atmosphere - Final Report* (Cambridge: MIT Department of Meteorology, 1949). See also **Nebeker** *Calculating the Weather*.

65 **Starr** *Final Report*; **V.P. Starr and R.M. White** *Balance Requirements of the General Circulation*, Geophysics Research Papers, No. 35 (Cambridge: 1954)

66 **G.B. Tucker** 'Some Developments in Climatology during the Last Decade', *Weather* **16** (1961): 391-400. In Nebeker's term, these studies were “theory-pull”. See **Nebeker** *Calculating the Weather*: Chapter 1.

67 Priestley's work was well known to the British meteorological community as he had worked at the Met Office until 1946 when he emigrated to Australia. **C.H.B. Priestley** *Interview by B. Morton* (Royal Meteorological Society Interview: April, 1988)

68 **N.K. Johnson** 'Milestones in a Century of Meteorology', *Weather* **5** (1950): 87-90.

available for investigations had to be devoted to forecasting research that would have immediate practical applications. Without support from the Met Office, it was not possible for a small number of academic meteorologists to perform such studies on their own. In the post-war reconstruction era in Britain, not every research programme could be carried out. The basic upper-air data were being collected in the Met Office, but were analysed using a synoptic climatology approach developed during the war. By charting the upper atmosphere, the Office produced mean monthly temperature and winds, with their variabilities, over the world as a whole.<sup>69</sup> These world charts contained useful information about the general circulation, yet they were used only for aviation forecasting or research into long-range forecasting.

At a more theoretical level, some progress was being made. Reginald Sutcliffe at the Met Office, both during and after the war, had examined how dynamical and thermodynamic equations could be used to link cyclone development to the pressure changes and other features on the upper-air weather maps, and had published his results in 1947.<sup>70</sup> Although Sutcliffe was chiefly concerned with the theoretical basis for weather forecasting and did not discuss the general circulation *per se* in his paper, his work could be applicable either to short-range forecasting or to climatology. In the universities, Eric Eady of Imperial College produced more mathematically oriented work on cyclone development. As Jule G. Charney had done in the US, he showed how the development of large-scale disturbances associated with emerging weather systems could be explained as a natural instability of the westerly zonal winds in the earth's atmosphere.<sup>71</sup> Through this work and others, Eady provided his version of a mathematical theory of the general circulation. Percival Sheppard, a professor of meteorology at Imperial, also became

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69 **Sawyer** 'Research in Synoptic and Dynamical Meteorology and in Climatology'.

70 **R.C. Sutcliffe** 'A Contribution to the Problem of Development', *Q. J. Roy. Meteor. Soc.* **73** (1947): 370-83.

The pressure coordinates that Sutcliffe suggested were later used in the development of the Met Office numerical weather prediction model. Sutcliffe joined the Met Office in 1927 after completing his PhD in mathematics. See **J.S. Sawyer** 'Reginald Cockcroft Sutcliffe', *Biogr. Mem. Fellows R. Soc.* **38** (1992): 345-58; **Sutcliffe** *Interview by J.M.C. Burton*.

71 **E.T. Eady** 'The Quantitative Theory of Cyclone Development', in *Compendium of Meteorology*, T.F. Malone (ed) (Boston: American Meteorological Society, 1951): 464-69; **P.D. Thompson** 'A History of Numerical Weather Prediction in the United States', *B. Am. Meteorol. Soc.* **64** (1983): 755-69. Eady studied mathematics in Cambridge before he joined the Met Office in 1937. After the war, he left the Met Office to pursue a PhD in mathematics at Imperial College. In 1948, he became a reader in physical climatology at Imperial's meteorology department. See **H. Charnock et al.** 'Obituary - E. T. Eady', *Q. J. Roy. Meteor. Soc.* **92** (1966): 591-92.

interested in the problem of general circulation from 1950 onwards.<sup>72</sup>

## Beginning of Numerical Weather Prediction

In 1946, John von Neumann, a prominent mathematician and pioneer of scientific computing, turned his attention to weather prediction as a test base for the newly developed electronic computer. From his experiences in weapons research, von Neumann well understood how important weather was to the military. He managed to persuade the US Navy to support his Meteorology Project, based at Princeton's Institute for Advanced Study (IAS), and in 1948 invited Jule Charney to become its leader.<sup>73</sup> By that time, Charney had developed the concept of quasi-geostrophic approximation and opened the possibility of using barotropic equations for numerical weather prediction (NWP).<sup>74</sup> The first 24-hour numerical forecast was successfully accomplished in 1950, using a barotropic model on the ENIAC computer.<sup>75</sup> A series of trial forecasts were made in the following years. In 1954, the Joint Numerical Weather Prediction Unit (JNWPU) was set up by the US Weather Bureau, the Air Force and the Navy. A year later, the JNWPU began to issue regular, real-time daily numerical weather forecasts using a barotropic model. As the computer technology and forecast algorithms improved, baroclinic models were also being constructed, though they were not yet used by the forecasters. A new era of numerical meteorology unfolded, realizing Richardson's dream.<sup>76</sup>

NWP developed somewhat slowly in Britain. When the war was over, Nelson Johnson was able to strengthen the research side of the Met Office. In 1948, he established the Forecasting Research Division alongside the Central Forecasting Office at Dunstable. Reginald Sutcliffe was appointed to lead the Division, which at first consisted of two

72 **P.A. Sheppard** 'The General Circulation of the Atmosphere', *Sci. Prog.* **40** (1952): 89-106.

73 See **Nebeker** *Calculating the Weather* : Chapter 10.

74 Charney assumed that large-scale atmospheric motions were predominantly horizontal, and that the pressure-gradient force was approximately balanced by the Coriolis force. This state was called *quasi-geostrophic*. Charney argued that, if this assumption was applied, the equations of atmospheric motions could be reduced to a single dynamically consistent equation, thereby imposing much less stringent conditions for computational stability. See *Ibid.*

75 *Ibid.*

76 *Barotropic* assumption did not take into account vertical wind shears and horizontal temperature gradients. By contrast, *baroclinic* models assumed that atmospheric disturbances could grow through conversion of potential to kinetic energy. These models were designed to yield a prediction at two or more levels. Though more elaborate than Richardson's numerical scheme, however, they used essentially the same numerical method of finite differences. See *Ibid.* and also **Thompson** 'A History of NWP in the US'.

branches – short-range and long-range forecasting research.<sup>77</sup> In the same year, the Meteorological Research Committee discussed the possible application of electronic computers in meteorology as the National Physical Laboratory planned the Pilot ACE computer project.<sup>78</sup> The Met Office's NWP research really began in 1950, when the Forecasting Research Division sent Fred Bushby to Imperial College to attend a course on numerical methods. Bushby was again sent to Cambridge in 1952, where the EDSAC computer had been developed. Soon he completed 2-level baroclinic atmospheric model equations, together with John Sawyer, head of Short-range Forecasting Research Branch.<sup>79</sup> The first 12-hour experimental forecast using this model produced encouraging results. Numerous trials followed in 1953-54.<sup>80</sup>

Interestingly, the Met Office meteorologists were more cautious about the future of NWP. In 1952, the annual report of the Office stated that, "there is still no evidence that the computer will do as well as the conventional forecaster".<sup>81</sup> While such a response might indicate the more generally conservative nature of the British Scientific Civil Service, it also reflected the institutional setting in which the Office scientists carried out their research.<sup>82</sup> Many of those involved in the Met Office's NWP project, including Sutcliffe, Sawyer and Bushby, were trained as mathematicians, and they appreciated the use of rigorous dynamical methods. Even so, they tended to be less concerned with theoretical elegance or with dynamical studies that had little impact upon practical applications. O.G. (later Sir Graham) Sutton, the new director of the Office, summarized this mood in 1953:

[E]ven if the methods were infallible (which it is not [*sic*]) the services of the

77 **Sutcliffe** *Interview by J.M.C. Burton*.

78 **J.S. Sawyer** 'Electronic Computing Machines and Meteorology', *Meteorol. Mag.* **81** (1952): 74-77.

79 After graduating from Imperial in mathematics in 1944, Fred Bushby first joined the RAF Meteorological Branch and a few years later became a scientific officer in the Met Office. See **B.J. Mason** 'Retirement of Mr F. H. Bushby', *Ibid.* **113** (1984): 29-31. His superior John Sawyer was also a mathematician. He graduated from Cambridge with distinction in Mathematical Tripos and then joined the Met Office in 1937. See **Taba** 'Bulletin Interviews - J.S. Sawyer'.

80 **M.K. Hinds** 'Computer Story', *Meteorol. Mag.* **110** (1981): 69-81.

81 **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1952): §4 Research and Development. Nebeker suggested that changeable weather in Britain might have made British meteorologists more sceptical. See **Nebeker** *Calculating the Weather*, at 211. Perhaps this could explain why some academic meteorologists were also critical of numerical weather prediction in Britain. But the Met Office scientists were sceptical about a particular way of doing numerical weather prediction, not the entire business of forecasting.

82 In other countries, the major numerical modelling groups at the time – the Meteorology Project and the JNWPU in the US and the Stockholm group in Sweden – were more or less independent research groups although they had various formal or informal relations with the governmental agencies. See **Nebeker** *Calculating the Weather*: Chapter 11.

meteorologist, as distinct from the mathematician, would still be required to ‘put into the weather,’ ...<sup>83</sup>

This pragmatic approach to dynamical meteorology in turn underpinned the modelling philosophy that particularly emphasized the physical realism of atmospheric models. For the Met Office scientists, barotropic treatment applying only in the mid-troposphere was intellectually stimulating and academically important, but not suitable for weather forecasting. At one of the Royal Meteorological Society discussion meetings in 1951, Sutcliffe, who was directing the Forecasting Research Division, argued that a future solution to the three-dimensional atmospheric problem would “derive little or nothing from the barotropic model”.<sup>84</sup> By contrast to the US and Swedish NWP groups, the Met Office worked on baroclinic models from the very beginning, thus permitting the prediction of both surface and upper-air charts.<sup>85</sup>

Partly due to this distinct style of numerical modelling, operational NWP in the Met Office was delayed for some time. However, more immediate concerns for the Office scientists were insufficient computing power and time. The Met Office did not then have its own electronic computer. The calculations were made using the LEO 1, a copy of EDSAC at J. Lyons & Co., a catering company, and later the Ferranti Mark 1 at Manchester University.<sup>86</sup> This significantly constrained the modelling work in the Office. In 1954, recognizing the potential of NWP, the Meteorological Research Committee requested that the Met Office should acquire an electronic computer for research purposes. Financial approval was given in the next year for the purchase of the Ferranti Mercury although the machine was actually installed only in 1959. With the Mercury computer, the 2-level Sawyer-Bushby model was extended to the 3-level Bushby-Whitelam model. As forecasting models became more advanced, attempts to introduce secondary physical

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83 **G. Sutton** 'The Development of Meteorology as an Exact Science', *Nature* **173** (1954): 1112-14. Sutton himself had been trained as a mathematician. After studying mathematics at Oxford, he became a lecturer in pure mathematics from 1926 to 1928. He then joined the Met Office and carried out mathematical investigations into atmospheric diffusion. After World War II, Sutton was appointed professor of mathematical physics at the Royal Military College of Science, where he remained until he succeeded Nelson Johnson as director of the Met Office in 1953. See **F. Pasquill et al.** 'Sir Oliver Graham Sutton', *Biogr. Mem. Fellows R. Soc.* **24** (1978): 529-46.

84 **R.C. Sutcliffe et al.** 'Dynamical Methods in Synoptic Meteorology - Discussion', *Q. J. Roy. Meteor. Soc.* **77** (1951): 457-73, quoted in **P. Lynch** 'Weather Forecasting: From Woolly Art to Solid Science', Invited contribution to Royal Meteorological Society Millennium Meeting, July 2000 ([http://www.maths.tcd.ie/~plynch/Publications/Woolly\\_art.pdf](http://www.maths.tcd.ie/~plynch/Publications/Woolly_art.pdf))

85 **Sawyer** 'Research in Synoptic and Dynamical Meteorology and in Climatology'.

86 **M.K. Hinds** 'Computer Story', *Ibid.* **110** (1981): 69-81.



processes were begun. In 1960, the Met Office launched a full-scale, real-time NWP experiment using the 3-level model.<sup>87</sup>

### **Towards General Circulation Modelling**

Weather prediction models were regional in their scope. The Met Office's trial forecasts in the 1950s usually covered Europe and the North Atlantic Ocean. In 1954, Norman A. Phillips, a meteorologist at the IAS, went further and suggested to Jule Charney the possibility of applying a baroclinic model to the general circulation of the atmosphere.<sup>88</sup> The idea was not to attempt a deterministic prediction of weather patterns over a long period, but rather to generate the statistical features of the atmosphere's general circulation. By mid 1955, Phillips was able to construct a 2-level quasi-geostrophic, hemispheric model that could sustain a stable integration for a reasonably long simulated time. Phillips' model, often referred to as the first general circulation model (GCM), was simplistic in many respects and the numerical calculations broke down after 30 days. Nevertheless, its results showed a remarkable agreement with the observed monthly and seasonal statistics of the real atmosphere.<sup>89</sup> Von Neumann was very impressed. A few months later, he organized a conference on "The Application of Numerical Integration Techniques to the Problem of General Circulation" in Princeton.<sup>90</sup> He subsequently submitted a proposal to the US Weather Bureau to form a new GCM research division. The new group, the General Circulation Research Section led by Joseph Smagorinsky,<sup>91</sup> was set up in the next month and began to develop a primitive equation GCM without barotropic or baroclinic assumptions.<sup>92</sup>

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<sup>87</sup> Ibid.

<sup>88</sup> **J.M. Lewis** 'Clarifying the Dynamics of the General Circulation - Phillips' 1956 Experiment', *B. Am. Meteorol. Soc.* **79** (1998): 39-60; **G.W. Platzman** 'The Atmosphere: A Challenge - Charney's Recollection', in *The Atmosphere: A Challenge - The Science of Jule Gregory Charney*, R.S. Lindzen *et al.* (eds) (Boston: American Meteorological Society, 1990): 11-88.

<sup>89</sup> **J. Smagorinsky** 'The Beginnings of Numerical Weather Prediction and General Circulation Modeling - Early Recollections', *Adv. Geophys.* **25** (1983): 3-37.

<sup>90</sup> **R.L. Pfeffer** (ed) *Dynamics of Climate - Proceedings of a Conference on the Application of Numerical Integration Techniques to the Problem of the General Circulation* (London: Pergamon Press, 1960)

<sup>91</sup> The General Circulation Research Section was first established as a joint project between the US Weather Bureau, the Air Force and the Navy. A year later, the Air Force and the Navy withdrew their support and the Weather Bureau assumed full responsibility. See **Smagorinsky** 'Early Recollections'.

<sup>92</sup> These primitive equations were, in fact, not so different from a set of seven partial differential equations that Richardson devised in the early 20th century. For Richardson's numerical forecast, see **Nebeker** *Calculating the Weather*; **O.M. Ashford** *Prophet or Professor? - The Life and Work of Lewis Fry Richardson* (Bristol: Hilger,

The British and American meteorological communities had had a close relationship. The Met Office and the US Weather Bureau routinely exchanged views on the problems arising in their numerical modelling work. Sutton, Sutcliffe and Sawyer were all aware of new developments initiated by Phillips' experiment. In 1956, Phillips also published his simulation results in a British journal, the *Quarterly Journal of the Royal Meteorological Society*, and became the first recipient of the Society's Napier Shaw Memorial Prize.<sup>93</sup> Phillips was invited to deliver a lecture in London, where his lecture was received with great interest by British meteorologists, including senior scientists from the Office.<sup>94</sup> Probably inspired by Phillips' work, the Met Office assigned a small group of scientists to embark on dynamical studies of the general circulation of the atmosphere. Hemispheric charts were produced and integrated to reveal the long waves in the atmospheric circulation. Andrew Gilchrist, a scientific officer at the Long-range Forecasting Branch, looked into the use of barotropic equations and whether large-scale atmospheric motions could be usefully represented in terms of spherical harmonic functions.<sup>95</sup> At the Climatological Research Branch, extending the work of Charles Priestley, Brian Tucker and his co-workers started to conduct observational studies of the transport of heat, momentum and moisture on a large scale.<sup>96</sup>

In the American universities, meteorology became firmly established by the early to mid 1950s.<sup>97</sup> The US Weather Bureau and governmental agencies such as the Air Force Cambridge Research Center commonly used their research expenditures for joint projects or contracts awarded to meteorologists in the universities, as von Neumann's Meteorology Project demonstrates. In Britain, the post war growth of meteorology was mostly confined to the Met Office. Even in the late 1950s, Imperial College was the only British university

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1985); **Ashford** 'Development of Weather Forecasting in Britain'.

93 **N.A. Phillips** 'The General Circulation of the Atmosphere - A Numerical Experiment', *Q. J. Roy. Meteor. Soc.* **82** (1956): 123-64; 'Discussions', *Q. J. Roy. Meteor. Soc.* **82** (1956): 535-39.

94 See **Phillips** 'Discussions'; **Lewis** 'Phillips' 1956 Experiment'.

95 Gilchrist held a first degree in mathematics from Glasgow and joined the Met Office in 1951. As already noted, the Met Office scientists were sceptical about the use of barotropic equations in weather models. But the scepticism did not apply to general circulation models. **A. Gilchrist** *Interview by the author* (June 9, 1999)

96 **Tucker** 'Some Developments in Climatology during the Last Decade'.

97 Meteorology had begun to gain academic recognition quite early in the US. Even before it went to war, six major universities had departments, or independent units, of meteorology. They were Cal Tech (1934), Chicago (1940), MIT (1928), NYU (1935), UCLA (1940) and Penn State (1935). The first five universities were actively involved in the military training programme and were known as the "Big Five". By 1951, however, 11 universities had meteorology departments while 4 others offered graduate-level training and 82 others offered undergraduate-level training in meteorology. See **Nebeker** *Calculating the Weather*; **D.D. Houghton** 'Meteorology Education in the United States after 1945', in *Historical Essays on Meteorology, 1919-1995*, J.R. Fleming (ed) (Boston: American Meteorological Society, 1996): 541-53.

that had an independent meteorology department. Elsewhere, meteorology training and research were carried out as minor part of the activities of geography, physics or mathematics departments.<sup>98</sup> Besides, like other branches of the British Scientific Civil service, and as part of the Air Ministry, the Met Office had a relatively hierarchical, closed culture. It tended to train its scientific staff in-house and did not encourage them to collaborate with the universities.<sup>99</sup> In 1958, the Secretary of State for Air set up a special committee with Lord Brabazon as chairman to review the work and organization of the Office.<sup>100</sup> The committee's recommendation led to a substantial reorganization and an increase in the research budget. This did not, however, bring about a close cooperation between the Met Office and the universities.

Outside the Met Office, Eric Eady at Imperial, in addition to elaborating his mathematical theory of the general circulation, was using the Manchester computer to develop a simple 2½-dimensional, baroclinic model of the atmosphere.<sup>101</sup> He also began to advocate going beyond empiricism in traditional studies of climate and applying the dynamical understanding of the general circulation to climatology.<sup>102</sup> Eady's theoretical and numerical work was highly respected by the meteorological community, especially those engaged in NWP research at Princeton's IAS. In fact, it was during a short visit to Eady in 1953 when Phillips first came to envisage the general circulation numerical experiment.<sup>103</sup> But for the reason already mentioned, and since his work was conceived by the Met Office as too theoretically oriented, Eady failed to secure the institutional and financial support that Jule Charney was enjoying in the US. Eady's modelling did not last long because of illness. Percival Sheppard, head of Imperial's meteorology department,

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98 It was the geography departments, which, as a result of World War II, began to invest more time on meteorology and climatology. Even in these departments, however, meteorology and climatology remained as minor topics. See **F.K. Hare** 'Geographical Aspects of Meteorology', in *Geography in the Twentieth Century: A Study of Growth, Fields, Technologies, Aims and Trends*, G. Taylor (ed) (London: Methuen, 1951): 178-95.

99 For the Met Office's in-house training, see **R.J. Ogden** 'Training at the Meteorological Office, 1936-71', *Weather* **47** (1992): 349-53. R.M. Goody, who was a reader in meteorology at Imperial in the early 1950s, recalled: " ... fundamentally, the Met Office weren't [sic] really interested in meteorologists, they wanted physicists or mathematicians to turn them into meteorologists themselves, that's what they were after ...". **R.M. Goody** Interview by F. Taylor (Royal Meteorological Society Interview: September 22, 1994)

100 **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1959)

101 **E.T. Eady** 'Note on Weather Computing and the So-called 2½-Dimensional Model', *Tellus* **4** (1952): 145-56.

102 **E.T. Eady** 'Climate', in *The Planet Earth*, D.R. Bates (ed) (London: Pergamon Press, 1957): 113-29.

103 See **H. Taba** 'The Bulletin Interviews - N. Phillips', *WMO Bulletin* **44** (1995): 215-24. Lewis, on the other hand, argued that Phillips first started to think about the general circulation experiment while he was involved in the barotropic forecasting experiments in Sweden. See **Lewis** 'Phillips' 1956 Experiment'.

urged his academic colleagues to follow up this work, but without much success.<sup>104</sup>

The Met Office carried on the synoptic analysis of upper-air climatology and dynamical-observational studies of large-scale atmospheric processes throughout the late 1950s. These studies proved useful in understanding certain features of the general circulation. And yet, there was still no strong institutional commitment to GCM development in the Met Office. With the highest priority given to short-range weather forecasting, the Office's research activities on other aspects of the subject were considerably inhibited by the limited availability of resources – above all, computing power and time.<sup>105</sup> The Ferranti Mercury computer had a relatively low calculation speed (*ca.* 3 *kFLOPS*) and did not much improve the Office's computational set up for numerical modelling. Gilchrist accordingly had to use the computer at Manchester for his early calculations.<sup>106</sup> Moreover, the Met Office preserved the generalist tradition of the British Civil Service and frequently moved its staff around across a range of different research and service divisions. Gilchrist, the only scientific officer working full-time on mathematical investigations of the general circulation, was transferred to the Central Forecasting Office in 1958. It took several years before he was released from other duties to work again on GCM development.<sup>107</sup>

## Atmospheric Radiation and Climate

While major advances in numerical modelling of the atmosphere were under way during the 1950s, there were also several important developments in atmospheric physics. In the US, utilizing the Northern Hemispheric upper-air data noted above, Henry G. Houghton at MIT and Julius London at New York University estimated the radiation budget of the globe.<sup>108</sup> This required detailed calculations of radiative transfer – *i.e.*, emission and absorption – by various atmospheric constituents such as ozone, water vapour and CO<sub>2</sub>,

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104 **J.S.A. Green** *Interview by the author* (August 3, 1999)

105 **Hinds** 'Computer Story'.

106 **Gilchrist** *Interview by the author*.

107 *Ibid.*

108 **Kutzbach** 'From Descriptive to Analytic'. Hunt *et al.* suggested that, although Simpson had brought remarkable insights into the problem of atmospheric radiation budget, Houghton and London significantly extended the scope of the study. Houghton's result was hemispheric, and London provided the first global radiation budget. See **Hunt et al.** 'A History of Presatellite Investigations of the Earth's Radiation Budget'.

which were extensively carried out by Lewis Kaplan at Princeton throughout the 1950s.<sup>109</sup> Outside the meteorological community, Gilbert Plass, an infrared physicist at Johns Hopkins, computed the atmospheric radiation flux for ozone and CO<sub>2</sub>.<sup>110</sup> In Germany, parallel studies were conducted by Fritz Möller at Mainz.<sup>111</sup> As for Simpson, Callendar and Humphreys, their results had implications in the study of climate. Plass in particular, following Callendar, attempted to renew the discussion of the connection between climate and atmospheric CO<sub>2</sub> in 1956.<sup>112</sup> His argument was not accepted by most scientists,<sup>113</sup> but indicated that global physical climatology might be developed. These works were followed in the early 1960s by the studies of Syukuro Manabe at the Weather Bureau's General Circulation Research Laboratory, who collaborated with Möller.<sup>114</sup>

In Russia, the study of the earth's energy balance was developed, from early on, in connection with the development of global physical climatology. Mikhail Budyko at the Main Geophysical Observatory began in the late 1940s to calculate annually the heat budget components for certain parts of the Soviet Union.<sup>115</sup> By 1956, he was able to publish a world atlas of the earth's heat balance which included net radiation maps for every month and for the entire year. Budyko's work became widely known to western scientists when his book "The Heat Balance of the Earth's Surface" was translated into

109 See **S.R. Weart** 'Global Warming, Cold War, and the Evolution of Research Plans', *Hist. Stud. Phys. Biol.* **27** (1997): 319-56, and references therein.

110 **G.N. Plass** 'The Influence of the 9.6 m Ozone Band on the Atmospheric Infra-Red Cooling Rate', *Q. J. Roy. Meteor. Soc.* **82** (1956): 30-44; 'The Influence of the 15 m Carbon Dioxide Band on the Atmospheric Infra-Red Cooling Rate', *Q. J. Roy. Meteor. Soc.* **82** (1956): 310-24.

111 **F. Möller** 'A Simplified Method of Computing the Radiation Balance in the Troposphere', in *Scientific Proceedings of the International Association of Meteorology* (London: Butterworths, 1956): 561-65.

112 Plass contended that doubling atmospheric CO<sub>2</sub> would lead to greater absorption of infrared radiation and raise the earth's surface temperature by 3.6 °C. See **G.N. Plass** 'The Carbon Dioxide Theory of Climatic Change', *Tellus* **8** (1956): 140-54.

113 For example, Kaplan strongly criticized Plass' results since they did not take into account the role of water vapour. **L.D. Kaplan** 'The Influence of Carbon Dioxide Variations on the Atmospheric Heat Balance', *Ibid.* **12** (1960): 204-08. See also **M.D.H. Jones and A. Henderson-Sellers** 'History of the Greenhouse Effect', *Prog. Phys. Geog.* **14** (1990): 1-18.

114 However, Manabe's real aim was to develop the radiative scheme for GCMs. See **F. Möller** 'On the Influence of Changes in the CO<sub>2</sub> Concentration in the Air on the Radiation Balance of the Earth's Surface and on Climate', *J. Geophys. Res.* **68** (1963): 3877-86; **S. Manabe and F. Möller** 'On the Radiative Equilibrium and Heat Balance of the Atmosphere', *Mon. Weather Rev.* **89** (1961): 503-32; **S. Manabe and R.F. Strickler** 'Thermal Equilibrium of the Atmosphere with a Convective Adjustment', *J. Atmos. Sci.* **21** (1964): 361-85. The US Weather Bureau's General Circulation Research Section changed its name to General Circulation Research Laboratory in 1959. A few years later, it changed again to Geophysical Fluid Dynamics Laboratory. See **P.N. Edwards** 'A Brief History of Atmospheric General Circulation Modeling', in *General Circulation Model Development - Past, Present, and Future*, D.A. Randall (ed) (San Diego: Academic Press, 2000): 67-90.

115 Both land and water surfaces of the earth were included, with the exception of the polar areas. For a brief review of Budyko's physical climatology, see **A. Kessler** *Heat Balance Climatology*, World Survey of Climatology (Amsterdam: Elsevier, 1985)



English by the US Weather Bureau in 1958.<sup>116</sup>

Atmospheric physics research was also actively pursued in British universities. In 1952, with the help of Charles D. Walshaw, Richard Goody in Cambridge formulated a simplified method for effective calculations of infrared radiative transfer in the atmosphere.<sup>117</sup> A year later, he joined Imperial College as a reader in meteorology, where he investigated the interaction between radiation and convection as well as the thermal structure of the stratosphere, until he left for Harvard in 1958.<sup>118</sup> Dobson and his collaborators at Oxford continued to be preoccupied with ozone measurements throughout the 1950s as they became intensely involved in creating a global network of ozone observing stations for the International Geophysical Year (IGY) 1957-58.<sup>119</sup> In the early 1960s, led by J.T. (now Sir John) Houghton, who later in 1983 became director-general of the Met Office, the Oxford group began to move away from ozone measurements.<sup>120</sup> Although its new research focus turned to the instrumental development for remote sounding from satellites, numerous works on atmospheric radiation calculations were also produced by Walshaw and his former student Clive Rodgers, who joined the group from Cambridge.<sup>121</sup>

All these research projects could have been readily linked to the study of climate. Goody's ideas, for instance, were later taken up by climate modellers.<sup>122</sup> Yet, at the time in Britain, atmospheric radiation experts in universities had little interest in the bigger

116 **M.I. Budyko** *The Heat Balance of the Earth's Surface* (Washington, DC: US Weather Bureau, 1958)

117 **R.M. Goody** 'A Statistical Model for Water Vapour Absorption', *Q. J. Roy. Meteor. Soc.* **78** (1952): 165-69. After graduating in physics from Cambridge, Goody worked at the Ministry of Aircraft Production for some time, and then came back to Cambridge for his PhD in Cavendish. His supervisor was Gordon Sutherland, a spectroscopist, and Goody had to learn meteorology by himself. In 1949, Goody got a research fellowship from Jesus College and began his research in Cavendish. C.D. Walshaw was then a PhD student. **Goody Interview by F. Taylor**.

118 **Goody Interview by F. Taylor**; **R.M. Goody** *The Physics of the Stratosphere* (Cambridge: Cambridge University Press, 1954)

119 The stations were equipped with Dobson spectrophotometers, and all new instruments had to be brought to Oxford for final calibration. Oxford group also carried out the study of atmospheric water vapour since World War II, but ozone was its core subject of study. See **Dobson** 'Forty Years' Research on Atmospheric Ozone'.

120 **J.T. Houghton Interview by the author** (October 4, 1999). John Houghton received his PhD from Dobson's group in 1955. After spending three years at the Royal Aircraft Establishment, Houghton returned to Oxford as a lecturer. As noted in Chapter 1, when the IPCC was established in 1988, he became the first chairman of its scientific assessment working group.

121 **C.D. Rodgers Interview by the author** (May 19, 2000)

122 The theoretical foundation for Goody's random band method is still being used by climate modellers when they parameterize in their GCMs the process of radiative transfer involving non-gray gases such as water vapour and CO<sub>2</sub>. As we will see in the next chapters, 1-D radiative transfer models played an important role in the later years in investigating the external radiative forcing effects on climate produced by greenhouse gases, aerosols and clouds.

questions concerning climate or even the global radiation budget. They were inclined to identify themselves as applied physicists rather than meteorologists, not to mention climatologists,<sup>123</sup> placing their emphasis almost entirely upon individual atmospheric phenomena. While they knew and valued Plass and Kaplan's infrared radiation research, the climatological aspects of their work were rarely discussed.<sup>124</sup> Guy Callendar kept on advocating the theory of enhanced greenhouse effect up to the early 1960s,<sup>125</sup> but only succeeded in drawing the attention of a few climatologists.<sup>126</sup> Budyko's energy balance climatology also failed to resonate with British atmospheric physicists. To them, Budyko's mathematical treatments were not rigorous enough. His insights into the physics of climate were, however interesting they might be, something to be appreciated by geographers, not by themselves.<sup>127</sup>

In the Met Office, atmospheric radiation studies were organized under the Physical Research Division by George D. Robinson, who returned to Kew Observatory after World War II.<sup>128</sup> In the early years, much work had to be done to improve the accuracy and precision of radiometers. The surface measurements of terrestrial radiation were instigated in 1946, along with aircraft and surface measurements of solar radiation.<sup>129</sup> The local radiation balance and albedo were then estimated from the observed data.<sup>130</sup> Radiation balance was one of the elements proposed for wide-scale study during the IGY. The Met Office took the initiative in expanding its radiation measurements, setting up a surface radiation station at Kew Observatory for IGY field experiments and instituting continuous

123 **Green** *Interview by the author*; **Rodgers** *Interview by the author*; **R.M. Goody** *Interview by the author* (January 12, 2001)

124 **G.N. Plass et al.** 'Discussions', *Q. J. Roy. Meteor. Soc.* **83** (1957): 272-75; **AMS/RMS Proceedings of the Toronto Meteorological Conference** (London: Royal Meteorological Society, 1954)

125 **G.S. Callendar** 'Can Carbon Dioxide Influence Climate?' *Weather* **4** (1949): 310-14; 'On the Amount of Carbon Dioxide in the Atmosphere', *Tellus* **10** (1958): 243-48; 'Temperature Fluctuations and Trends over the Earth', *Q. J. Roy. Meteor. Soc.* **87** (1961): 1-12.

126 See, e.g., **Brooks** 'Geological and Historical Aspects of Climatic Change'; **H. Landsberg and J.M. Mitchell** 'Correspondence', *Q. J. Roy. Meteor. Soc.* **87** (1961): 435-36; **H.H. Lamb and A.I. Johnson** 'Climatic Variation and Observed Changes in the General Circulation', *Geogr. Ann. A.* **41** (1959): 94-134.

127 **R.S. Harwood** *Interview by the author* (February 25, 1999). Indeed, it was physical geographers who most valued the work of Budyko.

128 Robinson held a PhD in physics from Leeds University. He began his career at the Met Office in 1936 as a research assistant to director of the Office, Sir George Simpson. At the time, Simpson was doing half-time research into atmospheric electricity at Kew Observatory. **G.D. Robinson** *Interview by E. Droessler* (AMS Tape Recorded Interview Project: June 27-28, 1994)

129 **G.D. Robinson** 'Physical Meteorology: Achievements and Prospects', *Meteorol. Mag.* **91** (1962): 319-26.

130 The earth reflects some of the incoming solar radiation back to space. The fraction of solar radiation reflected from the earth is called "albedo". **K. Warr and S. Smith** *Science Matters: Changing Climate* (Milton Keynes: Open University Press, 1993)

radiation measurements at sea.<sup>131</sup> The radiative properties of clouds and atmospheric aerosol were also investigated. The link between atmospheric physics and climatology was perhaps better recognized in the Met Office. Robinson suggested in 1962 that “a mapping of the flux of energy in its various forms as comprehensive as that mapping of temperature and wind” was needed.<sup>132</sup> But the Office’s radiation work was basically oriented towards observations and instrumentation. The development of a new physics-based climatology was left to dynamical meteorologists working on GCMs.

### **Establishment of the Dynamical Climatology Branch**

The British GCM research really started when the Met Office set up the Dynamical Climatology Branch in 1963. For many dynamical meteorologists, the GCM development was a natural extension to the previous NWP work. The Met Office’s Dynamical Climatology Branch was not created, however, on purely scientific grounds. It was also a by-product of growing political and economic interest in climate-related problems.

During the mid to late 1950s, the likelihood and ramifications of deliberate climate modification were being discussed in the US and Russia. In 1955, von Neumann pointed out that nuclear explosions could affect the earth’s albedo and eventually its weather and climate. He claimed that climate might be modified deliberately by changing the earth’s albedo in a similar way and warned that this could be used as a weapon of war.<sup>133</sup> Around the same time, it became public that Russian hydraulic engineers were discussing the prospect of constructing a large dam across the Bering Straits. They speculated that, by pumping water from the Arctic Ocean into the Pacific, and thereby drawing warm water up from the Atlantic, they might be able to remove the Arctic ice to warm Siberia.<sup>134</sup> While some scientists were more interested in the negative consequences of such plans,

131 See **Robinson** 'Physical Meteorology: Achievements and Prospects'. Robinson was a member of the Radiation Committee of the IAMAP (International Association of Meteorology and Atmospheric Physics) from 1947, and served as its secretary from 1957 to 1963. Partly through this committee, he got involved in planning and implementing the IGY. **Robinson** *Interview by E. Droessler*.

132 **Robinson** 'Physical Meteorology: Achievements and Prospects'.

133 **C. Kwa** 'The Rise and Fall of Weather Modification - Changes in American Attitudes Towards Technology, Nature, and Society', in *Changing the Atmosphere - Expert Knowledge and Environmental Governance*, C.A. Miller and P.N. Edwards (eds) (Cambridge: MIT Press, 2001): 135-65.

134 **S.R. Weart** 'Climate Modification Schemes', in *The Discovery of Global Warming*, <http://www.aip.org/history/climate> (recorded copy at American Institute of Physics, 2001): p. RainMaking-3 of 08/01/01 text.

British meteorologists, including the Met Office's senior scientists, were sceptical of the very possibility of deliberate climate modification. Sutton, director of the Office, contended in 1958 that "man cannot control weather on any substantial scale, and this state of affairs may persist for a long time to come, perhaps forever".<sup>135</sup> Nonetheless, the Office scientists had to respond to the government's concern over the issues.<sup>136</sup> Their answer was that a quantitative understanding of the general circulation of the atmosphere using numerical models should be achieved before any realistic assessments could be formed.

Another impetus for the development of GCMs, more important from the Office's point of view, was the prospect of dynamical long-range weather forecasting. The Met Office had long been concerned with the potential economic value of monthly and seasonal forecasts.<sup>137</sup> Despite the scepticism that J.M. Craddock experienced, the Office continued to develop long-range forecasting techniques based on empirical and statistical methods. Since the late 1950s, experimental monthly forecasts had been routinely produced using such techniques. However, although these forecasts showed tolerably good results, they were not issued to the governmental agencies and the public. The Office's senior scientists concluded that the empirical and statistical methods used for these forecasts were not scientifically grounded.<sup>138</sup> As NWP began to attest the power of dynamical methods, they increasingly came to believe that long-range forecasting should be best handled in the same way, but with a focus on the general circulation. In 1962, Sutcliffe, now director of research, wrote:

The question then arises whether success in long-range forecasting will come only by improving and extending the methods used for short-range forecasting ... success, if it is to come, requires a great deal more work to be done on the scientific study of the general circulation of the atmosphere.<sup>139</sup>

Ironically, the inception of the Met Office's GCM work owed much to the political

135 **G. Sutton** 'Man's Attempts to Control the Weather', *New Sci.* (September 4, 1958): 744-46.

136 John Houghton, director-general of the Met Office during 1983-91, suggested that the Air Ministry might have been interested in the climatic effects of nuclear war at the time. **Houghton** *Interview by the author*.

137 **R.C. Sutcliffe** 'Are Long-range Weather Forecasts Possible?' *New Sci.* **11** (1961): 211-13.

138 The 1959 annual report of the Met Office stated that "... no method of long-range forecasting is yet accepted as good enough for the public service ...". **British Met Office** *Annual Report, 1959*. See also **G. Sutton** 'Long-range Forecasts by the Meteorological Office', *Meteorol. Mag.* **93** (1964): 1-2.

139 Quoted from the *Directorate of Research* section of **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1962)

pressure to enhance old-style long-range forecasting activities. In the winter of 1962-63, the weather was exceptionally cold and had a marked effect on the British economy.<sup>140</sup> The severe winter had actually been predicted by the Met Office monthly forecast, but the results were as always not issued. On the other hand, the US Weather Bureau, using comparable techniques, had been issuing monthly weather forecasts, and in that particular year its success of predicting the cold winter attracted huge media attention both in the US and Britain. Facing public dissatisfaction, the British government and parliament intervened and instructed the Met Office to publish its monthly weather forecasts.<sup>141</sup> This opportunity was taken to reorganize climatological research in the Office. Rather than passively expanding its long-range forecasting activities, the Met Office divided the existing Climatological Research Branch (Met.O.13) into two new branches. The Met.O.13 was renamed as the Synoptic Climatology Branch, retaining the responsibility of the preparation of monthly forecasts, and the newly formed Dynamical Climatology Branch (Met.O.20) was commissioned to perform dynamical research into the general circulation and large-scale features of the atmosphere.<sup>142</sup>

By that time, Smagorinsky's group at the Geophysical Fluids Dynamics Laboratory (GFDL) had constructed a 2-level hemispheric, primitive equation GCM, incorporating the complete form of the equations of motion and model physics, and began to design a truly global, 9-level model.<sup>143</sup> The primitive equation GCMs were also being developed at UCLA's department of meteorology, the Lawrence Livermore National Laboratory, and the National Center for Atmospheric Research (NCAR).<sup>144</sup> Like those involved in US GCM groups, the Met.O.20 scientists considered it very important to take into account the effects of baroclinic scales on atmospheric motions, and acknowledged early on that the construction of a full primitive equation GCM should be their ultimate goal.<sup>145</sup> The problem was that most of the computing time available in the Met Office was being consumed by the Forecasting Research Division. Nor did the Met.O.20 have enough scientific manpower to undertake the complexity of the problem, having started as a small

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140 **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1963)

141 *Ibid.*

142 **R.C. Sutcliffe** 'Expansion of Meteorological Office Research in Dynamical Climatology', *Meteorol. Mag.* **93** (1964): 3-4.

143 **Smagorinsky** 'Early Recollections'.

144 **Edwards** 'A Brief History of Atmospheric General Circulation Modeling'.

145 **Gilchrist** *Interview by the author*.



research branch, initially comprising George Corby, Andrew Gilchrist and a few ancillary staff members.<sup>146</sup> Given the immense computational difficulties that would arise with GCMs, the Met.O.20 followed John Sawyer's suggestion to first build a simple 2-level quasi-geostrophic barotropic model in terms of Fourier series – *i.e.*, ordinary harmonics.<sup>147</sup> The model was designed to examine the behaviour of the planetary waves of low-wave number, in which smaller scale disturbances of the circulation were treated statistically. The Met.O.20 scientists thought that this approach would allow them to introduce the effects of geographic differences in topography and temperature into a relatively simple model.<sup>148</sup>

Even before the Met.O.20 was established, the shortage of computing power was by no means a trivial problem. The real-time tests using the 3-level Bushby-Whitelam model were well in progress. The Forecasting Research Division also initiated research into the primitive equation NWP models. In 1961, when the headquarters moved from Dunstable to Bracknell, the Met Office therefore made a decision to replace the Ferranti Mercury with a moderately powerful English Electric KDF9 (with a computing speed of 50 *kFLOPS*).<sup>149</sup> A new computer was installed in the summer of 1965, enabling two forecast runs per day. Later in the same year, however, the Met Office's numerical forecasts became officially operational, making full use of the KDF9 computer.<sup>150</sup> Other trial calculations, including that of a new Bushby-Timpson 10-level primitive equation NWP model, were fitted into the time available when the KDF9 was not heavily engaged by operational NWP. More often, they were executed using the much faster ATLAS computer, first at Manchester and later at the Rutherford Laboratory. The Met.O.20 was no exception. Its barotropic model still required several hours on the ATLAS computer for each integration, and had to be run at night.<sup>151</sup>

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146 The General Circulation Research Section, for instance, had nine scientists apart from ancillary staffs in 1956. They were all first class dynamical meteorologists, including Joseph Smagorinsky. See **H. Taba** 'The Bulletin Interviews - J. Smagorinsky', *WMO Bulletin* **32** (1983): 277-90. Corby also studied mathematics at London where he graduated with first class honour in 1941. He joined the Met Office in the same year and carried out forecasting research for many years before moving to the Met.O.20 as an assistant director

147 **G.A. Corby** *Personal communication with the author* (April 10, 2000)

148 **G.A. Corby and A. Gilchrist** *A General Circulation Experiment (Planetary-wave Model)*, Met.O.20 Technical Note, No. 1 (Bracknell: British Met Office, 1965)

149 **Hinds** 'Computer Story'.

150 *Ibid.*

151 **Gilchrist** *Interview by the author*.

## Expansion of Numerical Modelling at the Met Office

Operationalization of the Met Office NWP was chiefly due to the initiative of B.J. (now Sir John) Mason, the new director-general of the Office. Since his arrival in 1965, Mason had been very keen to establish the Met Office not only as an efficient weather service agency but also as one of the world's leading meteorological research institutions.<sup>152</sup> This was the era of the so-called “white heat” of the scientific and technological revolution in Britain.<sup>153</sup> Optimism for the potential of science and technology was at its height. They were portrayed as a vehicle for modernizing Britain. Within the first five years of his directorship, new research branches – *e.g.*, on cloud physics and geophysical fluid dynamics – were set up; many young science graduates were recruited into the Office, and more attention was directed to the importance of computing and telecommunication facilities and the potential of meteorological satellites.<sup>154</sup> Mason was also very enthusiastic about the use of the numerical modelling approach in atmospheric sciences. In the face of some scientists' reservations about the feasibility of real-time NWP, both inside and outside the Office,<sup>155</sup> he strongly insisted that the Met Office should begin to issue operational numerical weather forecasts.

These developments in the Met Office were further aided by growing international collaborations in meteorological services and research. Since the successful launching of the first artificial satellite, Sputnik-I, in 1957 and of the first man in space in 1961 by the Soviet Union, the US had been anxious to accelerate its own space programme and to bring the Russians into some forms of international scientific cooperation.<sup>156</sup> The US meteorological establishment quickly took this opportunity and prepared a position paper for the President's Science Advisory Committee (PSAC). As a result, President Kennedy, in his second State of Union address in May 1961, appealed for “at the earliest possible time a satellite system for world-wide weather observation”. Later in September that year, Kennedy addressed the UN General Assembly and called for “further cooperative efforts

152 John Mason was then professor of Cloud Physics at Imperial College. **B.J. Mason** *Interview by the author* (July 5, 1999); ‘Some Thoughts on the Future’, *Weather* **20** (1965): 273-74.

153 See, *e.g.*, **M.W. Kirby** ‘Blackett in the “White Heat” of the Scientific Revolution – Industrial Modernisation under the Labour Governments, 1964-1970’, *J. Oper. Res. Soc.* **50** (1999): 985-93; **H. Rose and S. Rose** *Science and Society* (Harmondsworth: Penguin, 1969)

154 **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1965-70)

155 **Green** *Interview by the author*, **G.D. Robinson** ‘Some Current Projects for Global Meteorological Observation’, *Q. J. Roy. Meteor. Soc.* **93** (1967): 409-18.

156 **R.G. Fleagle** ‘From the International Geophysical Year to Global Change’, *Rev. Geophys.* **30** (1992): 305-13.

between all nations in weather prediction and eventually in weather control".<sup>157</sup> The UN then adopted Resolution 1721 (XVI), which requested the World Meteorological Organization (WMO), in consultation with other international bodies, to draw up recommendations on appropriate arrangements to improve the meteorological services of member states and to advance the state of atmospheric sciences. The WMO responded in 1963 with an outline plan for the World Weather Watch (WWW), a global network of world and regional weather service centres and data processing and telecommunication systems. On the research side, the UN Resolution 1802 (XVII) was adopted in 1962, inviting the WMO and the International Council of Scientific Unions (ICSU) to devise an extensive programme of research in atmospheric sciences to complement the programmes being prepared by the WMO. From this was born the Global Atmospheric Research Programme (GARP).<sup>158</sup>

Both GARP and WWW took several years of lengthy negotiations over the planning and implementation programmes. The differences of interests among member states as well as rivalries between the government weather services and the academics had to be resolved. The final plan for the WWW was laid in April 1967 at the WMO 5<sup>th</sup> Congress.<sup>159</sup> Significant for the Met Office was that the Bracknell headquarters became a Regional Meteorological Center and a Regional Telecommunications Hub as part of the WWW.<sup>160</sup> This greatly helped Mason persuade the Treasury to provide the resources to procure an IBM 360/195 computer, to improve telecommunication facilities and to expand education and training schemes. It was GARP, however, which had a more direct relevance to the work of the Met.O.20. GARP was formally approved by the WMO and ICSU in October 1967.<sup>161</sup> The WMO/ICSU Joint Organizing Committee was subsequently established, with John Sawyer, director of research, being one of its members.<sup>162</sup> The objectives of GARP centred on the improved understanding of the physical and dynamical processes that would govern the global circulation of the

157 **M.A. Alaka** 'The Inception of the WWW', *WMO Bulletin* **37** (1988): 244-48.

158 **B. Bolin** 'Progress on the Planning and Implementation of the Global Atmospheric Research Programme', in *The Global Circulation of the Atmosphere*, G.A. Corby (ed) (London: Royal Meteorological Society, 1969): 235-55.

159 **Alaka** 'The Inception of the WWW'.

160 **B.J. Mason** 'Global Atmospheric Research Programme - Contribution to the Numerical Simulation and Prediction of the Global Atmosphere', *Earth-Sci. Rev.* **7** (1971): 165-86.

161 **Bolin** 'Global Atmospheric Research Programme'.

162 **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1967)

atmosphere. In the hope of providing a sound scientific basis for extended-range weather forecasting over periods of up to several weeks, their efforts were concentrated on the developments of physico-mathematical models of the global atmosphere, and of an optimum global observing system that would supply the input data for those models. The importance of studying climate was also recognized.<sup>163</sup> Through GARP, senior scientists of the Met.O.20, including George Corby and Andrew Gilchrist, participated in a series of international workshops and study conferences on global circulation modelling.

### The First Met Office GCM

By the time John Mason joined the Met Office, the Met.O.20 decided to develop its own primitive equation GCM.<sup>164</sup> After several months of examination, it became evident that no fruitful outcome could be achieved from a barotropic model.

Mason became more interested in GCMs later,<sup>165</sup> but he was supportive of the Met.O.20 from the beginning and provided it with several of the best young scientists in the Office.<sup>166</sup> With this extra manpower, the Met.O.20 boosted its GCM development. In 1966, numerical experiments were conducted to determine a suitable grid and vertical levels for a new model. The 5-level hemispheric form was finally adopted, with a horizontal resolution of  $3^\circ$  latitude  $\times$   $5^\circ$  longitude.<sup>167</sup> As for other GCMs, the Met.O.20 model simulated the major physical processes controlling atmospheric motions over a long time-scale. Many of these processes were still poorly understood, and in some cases there were no known laws for them. For some other processes, their scales were too small to be represented within the model grid. Thus, they had to be included as their statistical effects either by using the climatologically observed values, or by using “parameterizations” – *i.e.*, representations of the processes as functions of other fully resolvable variable without explicitly considering the details. For example, in the

163 **Mason** 'Global Atmospheric Research Programme'.

164 **Gilchrist** *Interview by the author*.

165 **B.J. Mason** 'Towards the Understanding and Prediction of Climatic Variations', *Q. J. Roy. Meteor. Soc.* **102** (1976): 473-98; 'Recent Advances in the Numerical Prediction of Weather and Climate', *Proc. R. Soc. Lon. Ser.-A* **A363** (1978): 297-333.

166 **Mason** *Interview by the author*; **Corby** *Personal communication with the author*.

167 **G.A. Corby et al.** 'United Kingdom Meteorological Office Five-Level General Circulation Model', in *General Circulation of the Atmosphere*, J. Chang (ed), Methods in Computational Physics, Vol. 17 (New York: Academic Press, 1977): 67-110; **A. Gilchrist** 'The Meteorological Office 5-Layer General Circulation Model', in *Report of the JOC Study Conference on Climate Models*, W.L. Gates (ed) (Geneva: WMO/ICSU, 1979): 254-95.

Met.O.20 model, sea surface temperatures were prescribed, along with seasonal and diurnal variations of solar radiation.

Trial computations were completed at each introduction of a new physical process. By 1967 about two-thirds of the programme was written.<sup>168</sup> A full programme became available in 1969, and simulations using the Met.O.20 5-level GCM were started. In order to achieve computing efficiency, compromises had to be made concerning the model resolution, effectiveness of parameterizations and length of integration of the model. At first, the calculation was carried forward to 35 simulated days.<sup>169</sup> The results showed an encouraging degree of realism – the model successfully simulated the major features of the general circulation as well as the seasonal distributions of temperature, pressure, wind and rainfall observed in the real atmosphere. In 1970, the integration was extended to 60 days.<sup>170</sup> The careful analysis of the 60-day simulation was carried out in the following years, leading to a number of changes in the model which were mainly of a purely mathematical kind, and some concerning the mathematical representation of the physical processes.

GCM was thought to provide a theoretical tool for assessing the length of time over which short-range weather forecasts would ultimately be possible and the observational network that would be needed to achieve them.<sup>171</sup> The importance of GCM development was also being stressed in the context of a growing interest in deliberate or inadvertent climate modification. The US National Academy of Sciences (NAS) appointed a Panel on Weather and Climate Modification in 1963 to review the status and activities in the field, and issued a final report in 1966.<sup>172</sup> GFDL, the leading GCM research group, was involved in the assessment, although GCMs were not actually used. These events highlighted various potential applications of GCMs. There was some general concern that Britain might fall behind in state-of-the-art meteorological research.<sup>173</sup> The Met.O.20's primary aim at this stage, however, was to develop a GCM for investigating anomalies of

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168 **British Met Office** *Annual Report, 1965-70*.

169 **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1969)

170 **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1970)

171 **Bolin** 'Global Atmospheric Research Programme'.

172 **US NRC** *Weather and Climate Modification - Problems and Prospects, Vol. 2: Research and Development* (Washington, DC: National Academy of Sciences, 1966). See also **Kwa** 'The Rise and Fall of Weather Modification'.

173 **R.C. Sutcliffe** 'Control of Weather and Climate', *Nature* **210** (1966): 459-60.



the presently observed climate. The Met.O.20 scientists believed that this was a logical way to proceed in the early stage of model development. For testing against observable and well-documented atmospheric phenomena would enable model characteristics and possible shortcomings to be identified, and the performance of GCMs in any future perturbed simulations should be judged against the control ones. But the tendency to lay more emphasis upon relatively short-term and regional natural climatic variations was, as has already been noted, closely related to the Met Office's interest in the prospect of long-range forecasting for about a month ahead by means of dynamical methods.

The Met.O.20's emphasis on the detailed variations of the presently observed climate was visible in its approaches to some of the most difficult parts in developing a GCM – for example, the transfer of heat by radiation. It was generally believed that the net radiative flux at any level in the atmosphere depended on the temperature and concentration of the radiatively active constituents at all levels. Hence, while the theory had been available to account for many of these processes, its full exploitation in numerical simulations would be computationally very expensive.<sup>174</sup> Furthermore, if the effects of radiation were specified climatologically, the feedback aspects of the processes would not be fully taken into account. The Met.O.20 took the middle-way approach, and in collaboration with Walshaw and Rodgers at Oxford, formulated a non-interactive scheme based on long-wave cooling and solar heating rates derived from mean climatological data.<sup>175</sup> This approach omitted – or at best only indirectly and insufficiently incorporated – the effects of cloud on the radiation field.<sup>176</sup> So it was seen as having disadvantages in simulating climate perturbations due to external forcings such as changes in solar radiation, changes in the composition of the atmosphere, land-use changes, or volcanism.<sup>177</sup> But the scheme was very fast and effective, giving good results when radiation was not the essence of the problem – *i.e.*, the simulation of natural climate, especially in the troposphere.<sup>178</sup>

174 **Corby et al.** 'UK Met Office 5-Level GCM'.

175 **C.D. Rodgers and C.D. Walshaw** 'Computation of Infra-Red Cooling Rate in Planetary Atmospheres', *Q. J. Roy. Meteor. Soc.* **92** (1966): 67-92.

176 **Corby et al.** 'UK Met Office 5-Level GCM'.

177 *External forcings* refers to the forcings that are imposed on the atmosphere from outside the domain of interest.

178 Perhaps this point should not be emphasized too strongly. The Rodgers-Walshaw radiation scheme could still be applied to the study of climate perturbations, and was later used by Manabe at GFDL. See **S. Manabe** 'Estimates of Future Change of Climate Due to the Increase of Carbon Dioxide Concentration in the Air', in

The Met.O.20's emphasis on simulation of the presently observed climate continued to dominate its GCM development throughout the 1970s. The rationale was that, "before the model can represent changes in climate correctly, it must reproduce the present climate correctly".<sup>179</sup> In a similar way as in the NWP work, this reflected both a cautious and conservative style of the British Scientific Civil Service and the Met Office's pragmatic approach in dynamical meteorology.

Even with the whole power of the ATLAS computer, running a hemispheric model was too expensive. The 60-days integration of the Met.O.20 5-level GCM took about 120 hours.<sup>180</sup> With the new IBM 360/195 computer (with a computing speed of  $4 \times 10^3$  *kFLOPS*) installed in 1971, however, the same integration took less than ten hours.<sup>181</sup> The Met.O.20 could therefore pursue its modelling work, for the first time, relatively free from computational constraints. The model was now run both on a hemispheric and a global scale.<sup>182</sup> In the course of re-writing the programme for the IBM 360/195, the Met.O.20 introduced a new grid and finite differences system, in which the zonal grid length was more nearly constant than in the previous system. A new higher-resolution global 11-level GCM also began to be developed.<sup>183</sup> By the mid 1970s, the Met.O.20 expanded to have five independent research groups of its own, covering 5- and 11-level GCM developments, tropical meteorology, stratospheric modelling and global data processing, with more than twenty scientific staff members.<sup>184</sup> GCM was finally becoming a major part of British climate science.

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*Man's Impact on the Climate*, W.H. Matthews *et al.* (eds) (Cambridge: MIT Press, 1971): 249-64. Furthermore, the Met Office was deliberately trying not to duplicate the work of GFDL. The annual report of 1967 stated: "Some extremely successful work of this kind has already been carried out by research groups in the U.S.A. but there are so many opportunities for differing treatments in the design of a model and so many variants of the possible experiments that may be conducted with it that there is little likelihood of simply duplicating the work. Furthermore, independent approaches by different research groups are desirable to ensure that the success (or failure) of some experiment does not depend on some arbitrary assumption adopted within the formulation of the model without realization of its significance." **British Met Office** 'Special Topic - The General Circulation of the Atmosphere', in *Annual Report of the Meteorological Office, 1967* (London: HMSO, 1967): 53-59.

179 **A. Gilchrist** 'Concerning General Circulation Models', *Meteorol. Mag.* **108** (1979): 35-51.

180 **G.A. Corby et al.** 'A General Circulation Model of the Atmosphere Suitable for Long Period Integrations', *Q. J. Roy. Meteor. Soc.* **98** (1972): 809-32.

181 **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1971)

182 **A. Gilchrist et al.** 'A Numerical Experiment Using a General Circulation Model of the Atmosphere', *Q. J. Roy. Meteor. Soc.* **99** (1973): 2-34.

183 **British Met Office** *Annual Report, 1971*.

184 **P.R. Rowntree** *Interview by the author* (October 4, 1999)

## Chapter 3. Early Development of Climate Science and Politics

### Hubert Lamb and Historical Climatology

Throughout the 1950s, there had been little systematic research on climate change in the Met Office. The Office had the Climatology and Agriculture Division, which was nominally under the directorate of research. But this division was essentially on the service side, and its main function centred upon the production, archiving and dissemination of regional climatic records for the Office's operational activities. The scientific staff of the division devoted themselves chiefly to investigations of the upper air. Synoptic climatological analyses were carried out on a large-scale, charting the upper atmosphere to obtain mean monthly temperatures and winds, with their variabilities, over the world. As described in the previous chapter, these studies were also supplementary to the Office's aviation forecasting and long-range weather forecasting. Just as for other sub-disciplines of meteorology, climatological research that was of little direct use to the operational activities was largely left to the initiative and responsibility of individual scientists. For example, C.E.P. Brooks, a leading climatologist at the Met Office, did much of his pioneering work on climate change in his off-duty time.<sup>1</sup> In 1954, a new Climatological Research Division was formed from a part of the Climatology and Agriculture Division (*i.e.*, the World Climatology, Upper Air Climatology and Library Editing Branches),<sup>2</sup> but the basic organization of the Office's research in climatology remained unchanged.

It was Hubert Lamb who pioneered the studies of past climates and climate change at the Met Office. Lamb himself was drawn to the issue rather accidentally. In 1954, after working at the Long-range Forecasting Research Branch for several years, Lamb was appointed new Head of the Library Editing Branch. Before Lamb's transfer took place,

<sup>1</sup> Although Brooks is now usually remembered as the author of 'Climate through the Ages' and 'English Climate', his main contribution to the Met Office at the time was the development of statistical methods in meteorology and upper air climatology. See **A.H.R. Goldie** 'Obituary - C. E. P. Brooks', *Q. J. Roy. Meteor. Soc.* **84** (1958): 202-03; **C.E.P. Brooks and N. Carruthers** *Statistical Methods in Climatology* (London: HMSO, 1944)

<sup>2</sup> The British Climatology and Agricultural Meteorology Branches were moved to the Climatological Services Division, which was also newly created. See **British Met Office** *Annual Report of the Meteorological Office*

however, his predecessor was advised to return to his old post because of his illness. Lamb thus became attached temporarily to the Climatological Research Division, dealing with overseas climatological inquiries. This not only left Lamb relatively free to conduct his own research – he was almost forgotten for two years – but gave him an unexpected opportunity to discover the immense amount of meteorological observations over the world, especially over the North Atlantic and Europe, archived at the Met Office Library. He soon realized that, using these unstudied data, he could produce monthly mean pressure maps for each January and July going back to as far as 1750.<sup>3</sup> Lamb also discovered that the Archive held ships' logs from the 19<sup>th</sup> century onwards, and that these contained data on sea surface temperatures. When, in 1955, the Royal Meteorological Society announced a competition on the subject of climate variation for a prize essay in 1959, Lamb managed to persuade his senior scientists to obtain an official permission to pursue this new research into the reconstruction of past climates.<sup>4</sup>

Lamb did not belong to the new generation of scientific officers trained in physics or mathematics. Before joining the Met Office in 1936, Lamb was trained in Cambridge, where he initially entered the Natural Science Tripos but ended in geography.<sup>5</sup> While he began to develop interdisciplinary thinking from his geography background, his approach was different from that adopted by traditional geographer-climatologists such as Gordon Manley. Lamb started his Met Office career as a forecaster at Croydon Airport for transatlantic flights. At the outbreak of World War II, he was sent to the Irish Meteorological Service and went on there to produce transatlantic aviation forecasts.<sup>6</sup>

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(London: HMSO, 1954-55)

3 H.H. Lamb and A.I. Johnson 'Climatic Variation and Observed Changes in the General Circulation', *Geogr. Ann. A.* **41** (1959): 94-134.

4 Lamb did not win the major prize in 1959. S.K. Runcorn, a professor of physics at Newcastle, won the prize for his essay on the longer-term climate developments and geomagnetism. Lamb's work was, however, granted honourable mention as *proxime accessit* (runner up). See H.H. Lamb *Through All the Changing Scenes of Life - A Meteorologist's Tale* (East Harling: Taverner, 1997)

5 Lamb did not even finish Geography Part II. He only got a Second Class Honours degree in Geography Part I. At the time of severe unemployment, he was perhaps lucky to get a job at the Met Office with such qualifications. It is interesting to note that his grandfather was a prominent hydrodynamicist, Horace Lamb, and his father was a professor of mechanical engineering at East London College (later renamed as Queen Mary College). Hubert Lamb later suggested that his grandfather's fame might have helped him join the Met Office. He was interviewed by Sir George Simpson, director of the Met Office, who had been Horace Lamb's pupil at Manchester. Also noteworthy is that Lamb knew Lewis F. Richardson quite well from his boyhood. Richardson's son was one of Lamb's closest school friends. But it was Richardson's Quakerism, not his mathematical genius, that had a strong influence on Hubert Lamb. See *Ibid.*

6 As World War II began to unfold, Lamb was first instructed to work on the meteorology of gas spraying. Being a Quaker, he resigned from the Office, but was eventually reinstated and posted to the Irish Meteorological Service. See *Ibid.* and also H.H. Lamb *Interview by R.A.S. Ratcliffe* (Royal Meteorological Society Interview: April 25, 1985)

Through these works, he became acquainted with a synoptic understanding of large-scale atmospheric circulation patterns. He was particularly influenced by Tor Bergeron's new text<sup>7</sup>, which he used in training Irish recruits for aviation forecasting. During the late 1940s, at the Long-range Forecasting Research Branch, Lamb formulated a classification system for daily weather maps of the British Isles on the basis of atmospheric circulation types.<sup>8</sup> By the same token, his reconstruction of mean pressure maps from the 18<sup>th</sup> century was designed to reveal the changes in atmospheric circulation patterns. These changes were then discussed with respect to the known changes in temperature, ice cover, ocean currents, and rainfall.<sup>9</sup> Hence, although his method was descriptive, inductive and qualitative, Lamb sought to integrate the meteorologists' perspective into his approach.

On returning from Ireland after the war, Lamb volunteered for the *Balaena* whaling expedition in the Antarctic before he was posted to the Long-range Forecasting Research Branch in 1948.<sup>10</sup> His job was to produce weather forecasts for the whaling ship. During the expedition, Lamb found that his weather maps over the Southern Ocean showed a difference between the outward and homeward voyages. This was in contrast to the conventional view that the sharp discontinuities between cold and warm water in the Southern Ocean would be invariant. Due to incomplete observations and other limitations, Lamb could not properly examine the issue, but it was through this experience that he first came to question the idea that climate was practically invariant.<sup>11</sup> Later, back in the Met Office, he had abundant meteorological observations in the library. Lamb's meticulous reconstruction of past weather maps seemed to indicate that during the past one or two hundred years there had been noticeable changes in the intensity and pattern of the general circulation and in sea surface temperatures.<sup>12</sup> Even though his observational records were limited to the North Atlantic and Europe, Lamb became convinced of what he had already suspected – climate did really change world-wide on time-scales of significance to humankind. As has been noted, the majority of meteorologists, and even some climatologists, at the time accepted the static view of climate; that is, climate could be

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7 This book was essentially a collection of Tor Bergeron's lectures given in Russia. It had been written in German by Bergeron's Czech student, G. Swoboda. See **Lamb** *Through All the Changing Scenes of Life*, at 89.

8 **A. Perry and J. Mayes** 'The Lamb Weather Type Catalogue', *Weather* **53** (1998): 222-29.

9 See, for example, **Lamb and Johnson** 'Climatic Variation and Observed Changes in the General Circulation'.

10 **Lamb** *Through All the Changing Scenes of Life*.

11 *Ibid.*

12 **Lamb and Johnson** 'Climatic Variation and Observed Changes in the General Circulation'.



assumed to be constant except for geological time-scales. In Lamb's view, his pressure maps directly challenged the prevailing paradigm.

The realization of changing climate, in turn, led Lamb to investigate its causes. He reviewed a range of theories and hypotheses including the CO<sub>2</sub> theory of climate change. What interested him most was the connection between volcanic eruptions and past cold climates.<sup>13</sup> Following the previous studies by William J. Humphreys and Harry Wexler of the US Weather Bureau and others, Lamb believed that volcanic dust could affect the transfer of radiation through the atmosphere and cool the climate. He painstakingly listed and classified the known events of volcanic eruptions since 1500 and examined the correlation between the estimated dust emission levels and cooler years.<sup>14</sup> Lamb's interest extended further back in time when he was invited by Harry Godwin of Cambridge Botany School, a pioneer on pollen analysis, to join his Quaternary discussions.<sup>15</sup> The general circulation of the atmosphere was still at the core of his thinking, but he broadened his understanding by taking a more interdisciplinary approach.

Lamb also began to correspond with influential members of the international climatological community such as Hermann Flohn and Richard Scherhag in Germany.<sup>16</sup> By the early 1960s, he became well-known internationally in the field of studies of climate variation and past climates. This nonetheless did not change the Met Office's attitude towards climate research.

### Growing Interest in Natural Climate Variability

Undoubtedly, the Met Office meteorologists recognized the socio-economic importance of climate. The Met Office was routinely providing climatological information and advice to agriculture, construction, electricity and gas industries, as well as to the military and government agencies. Climate was understood early on as one of the natural resources

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<sup>13</sup> Ibid.

<sup>14</sup> H.H. Lamb 'Volcanic Dust in the Atmosphere - With a Chronology and Assessment of Its Meteorological Significance', *Philos. Tr. R. Soc. S.-A* **266** (1970): 425-533.

<sup>15</sup> Lamb *Through All the Changing Scenes of Life*. See also H.H. Lamb 'Palaeoclimatology', *Meteorol. Mag.* **92** (1963): 246-49.

<sup>16</sup> Lamb *Through All the Changing Scenes of Life*.

that were vital to the national economy.<sup>17</sup> Yet the meteorologists tended to shy away from the practical implications of climate variations. They conceded that climate varied through time, but their static view of climate meant that the variations within the historical period were taken as no more than random fluctuations. Given the climatic data of several decades, they assumed, such short-term fluctuations would be statistically evened out. The averaged climatological information, “climate normal”, was thought to be enough to serve the long-term planning objectives. There was no perceived need, therefore, for incorporating new studies of climate variation into the existing climatological practices.

When the issue of climate variations was discussed, the focus was on their immediate impacts. For instance, L.P. Smith, an agricultural meteorologist at the Met Office, argued that the study of climate data alone would be of little value. He wrote in 1961:

If ... we examine recent climatic variations through the eyes of a statistician, we may be forced to conclude that none of them are “significant”, that there has been no major change in the causes of our climate and that the variations are merely equivalent to a different set of solutions to a series of semi-indeterminate thermodynamical equations.<sup>18</sup>

His point was not that climate variations could be ignored but that they should be measured only with respect to their effect on specific human activities, not with reference to the details of climate data. In presenting such an argument, Smith implicitly accepted the static conception of climate and assumed that it would not be feasible to identify climate variations.

Hubert Lamb thought otherwise. He, too, was concerned with the effect of climate variations. His geography-based interdisciplinary background, together with his forecasting experience, helped him readily connect his scientific work with societal needs. Delving into a myriad of historical documents concerning climatic events, he also learned that climate variations had long had considerable impact on human society.<sup>19</sup> However, Lamb’s research developed almost independently of the Met Office’s existing

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<sup>17</sup> At the time, climate was more often approached within the framework of regional or national, rather than global, natural resource issues. In the Met Office, for instance, the British Climatology Branch was given far greater priority than the World Climatology Branch. See **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1951-60)

<sup>18</sup> L.P. Smith ‘Measuring the Effects of Climatic Changes’, *New Sci.* **12** (1961): 608-11.

<sup>19</sup> Lamb suggested that he was influenced by English historians such as G.M. Trevelyan. See H. Taba ‘The

climatological practices. While he advocated the practical value of climate research, he was vocally critical of the traditional, static view of climate. As early as in the late 1950s and early 1960s, he suggested that long-term social and economic planning should not rely on the assumption of climate constancy or on naive extrapolation from recent climate trends.<sup>20</sup> Instead, he argued, a new and better understanding of climate change was necessary.

To some extent, it was understandable that meteorologists and the general public were indifferent to climate variations. From the early 20<sup>th</sup> century to the 1940s, there appeared to be an apparent warming trend that was felt to make life easier in many parts of the world.<sup>21</sup> Even a proponent of the CO<sub>2</sub>-induced warming hypothesis, Guy S. Callendar, thought that a sustained warming trend would be beneficial to Britain.<sup>22</sup> From the 1950s onwards, the perception gradually began to change. A world-wide cooling seemed to have set in, reversing the early warming trend, at first slightly but more visibly in the 1960s.<sup>23</sup> In addition, extreme weather events were reported more frequently and more intensely both in Europe and all over the world.<sup>24</sup> These developments consolidated Lamb's conviction of the reality of climate change and its societal significance. Throughout the 1960s, he kept arguing that more attention should be paid to the increase in natural climate variability.

The occurrence of climate extremes in the 1960s could still be interpreted in terms of

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Bulletin Interviews - H. H. Lamb', *WMO Bulletin* **43** (1994): 99-110.

20 See, e.g., **H.H. Lamb** 'The Changing Climate', *New Sci.* (October 22, 1959): 740-44; **H.H. Lamb and A.I. Johnson** 'Climatic Variation', *Meteorol. Mag.* **91** (1962): 170-71.

21 However, Europe was severely hit by a series of extremely cold winters in 1941-43 and 1947. See **W.J. Burroughs** *Does the Weather Really Matter? - The Social Implications of Climate Change* (Cambridge: Cambridge University Press, 1997): Chapter 3.

22 **G.S. Callendar** 'Temperature Fluctuations and Trends over the Earth', *Q. J. Roy. Meteor. Soc.* **87** (1961): 1-12.

23 **J.M. Mitchell** 'Recent Secular Changes of Global Temperature', *Ann. NY. Acad. Sci.* **95/1** (1961): 235-50.

There were relatively few sites around the globe from which temperature figures were taken and averaged to give the world-wide trend. It could thus be argued that such trends, whether warming or cooling, were artificial.

24 Some of the weather extremes reported in the 1960s were as follows:

1960-69:	Driest decade in central Chile since the 1770s and 1790s.
1962-63:	Coldest winter in England since 1740.
1962-65:	Driest four-year period in the eastern United States since records began in 1738.
1963-64:	Driest winter in England and Wales since 1743; coldest winter over an area from the lower Volga basin and Caspian Sea to the Persian Gulf since 1745.
1965-66:	Baltic Sea completely ice-covered.
1968:	Arctic sea ice half surrounded Iceland for the first time since 1888.
1968 ~	Severest phase thus far of the prolonged drought in the Sahel zone of Africa, surpassing all recorded (twentieth-century) experience.

From **H.H. Lamb** *Climate: Present, Past and Future - Vol. 2 Climatic History and the Future* (London: Methuen, 1977); *Climate, History and the Modern World*, 1<sup>st</sup> edition (London: Methuen, 1982); *Weather, Climate &*

random fluctuations. Nevertheless, public interest in natural climate variability was growing. Lamb was increasingly invited to do radio interviews and asked to give lectures on climate variation and its history.<sup>25</sup> Academic interest in the topic also grew, and a series of international conferences and workshops on past climates and climate change ensued. In January 1961, the New York Academy of Sciences and the American Meteorological Society jointly held a conference on “Solar variations, Climatic Change and Related Geophysical Problems”.<sup>26</sup> This was the first international conference bringing together scientists from diverse disciplines related to climate change. In October of the same year, with the request by the Food and Agricultural Organization (FAO), the United Nations Educational, Scientific, and Cultural Organization (UNESCO) and the World Meteorological Organization (WMO) arranged a symposium on “Changes of Climate” in Rome, Italy.<sup>27</sup> In 1963, the NATO “Palaeoclimates” conference was held in Britain.<sup>28</sup> Two years later, under the auspices of the US National Academy of Science (NAS), the National Center for Atmospheric Research (NCAR) and the International Union for Quaternary Research (INQUA) organized a symposium in Boulder, Colorado, which specifically dealt with the “Causes of Climatic Change”.<sup>29</sup> The Royal Meteorological Society’s first international conference on past climates was held in 1966.<sup>30</sup>

Dynamical meteorologists slowly began to participate in the discussions of past climates and climate variations. In the 1950s, the presentations of dynamical meteorologists and climatologists were rather clearly separated into different sessions at international meteorological conferences, such as the 1953 Toronto conference jointly held by the American Meteorological Society and the Royal Meteorological Society, and

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*Human Affairs - A Book of Essays and Other Papers* (London: Routledge, 1988)

25 **Lamb** *Through All the Changing Scenes of Life*.

26 **R.W. Fairbridge** (ed) *Solar Variations, Climatic Change, and Related Geophysical Problems*, Ann. NY. Acad. Sci. **95/1** (New York: New York Academy of Sciences, 1961). The conference participants ranged from E.B. Kraus, H.C. Willet, H. Flohn, H.H. Lamb, G. Manley, J.M. Mitchell to C. Emiliani. Dynamical meteorologists of the new generation were largely absent, but it is noteworthy that a physicist G.N. Plass presented his results on the effects of atmospheric CO<sub>2</sub> on climate. See Chapter 4 for more about the work of Plass.

27 See **UNESCO/WMO** *Changes of Climate - Proceedings of the UNESCO/WMO Rome Symposium*, Arid Zone Monogr. XX (Paris: UNESCO, 1963). The FAO was concerned with the potential effects of a global cooling trend on the world’s food production.

28 **A.E.M. Nairn** (ed) *Problems in Palaeoclimatology - Proceedings of the NATO Palaeoclimates Conference* (New York: Interscience Publishers, 1965)

29 **J.M. Mitchell** (ed) *Causes of Climatic Change - A Collection of Papers Derived from the INQUA-NCAR Symposium*, Meteorological Monographs, Vol. 8, No. 30 (Boston: American Meteorological Society, 1968)

30 **J.S. Sawyer** (ed) *World Climate from 8000 to 0 BC - Proceedings of the International Symposium* (London: Royal Meteorological Society, 1966)

the 1954 meeting of the International Association of Meteorology.<sup>31</sup> By the early to mid 1960s, this was no longer the case. The UNESCO/WMO “Changes of Climate” symposium was attended by a number of prominent dynamical meteorologists including Reginald Sutcliffe and John Sawyer of the Met Office and Edward N. Lorenz of MIT. The participants of the NATO palaeoclimatology conference included Percival Sheppard, Head of Imperial College’s Meteorology Department. The NCAR/INQUA symposium was from the beginning designed to facilitate dialogue among palaeoclimatologists, geologists and meteorologists. Yale Mintz of UCLA and Lorenz, among other dynamical meteorologists, presented on the theoretical aspects of climate change.

### **Historical *versus* Dynamical Approaches**

The growing participation of dynamical meteorologists does not necessarily mean that they fully supported the work of climatologists. For them, the work of geologists and palaeoclimatologists on the causes of long-term climate change appeared to be “endless speculation”.<sup>32</sup> Nor were they content with previous research into climate change of the recent historical period. Reporting on the 1961 UNESCO/WMO symposium, Sutcliffe, then director of research at the Met Office, wrote:

The broad impression created by the meeting is that, for the period of instrumental record, the subject, which has got to a certain descriptive stage by pure empiricism, is in danger of languishing for want of ideas.<sup>33</sup>

Such a danger, dynamical meteorologists believed, could only be overcome by employing theoretical, experimental and dynamical-observational approaches to the general circulation of the atmosphere. Their ultimate goal was the construction of general circulation models (GCMs), which by then were being actively developed in various research centres such as the Geophysical Fluids Dynamics Laboratory (GFDL) in the US and the Met Office in Britain.

The Met Office meteorologists were particularly critical of Lamb’s historical

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<sup>31</sup> **IUGG** *Scientific Proceedings of the International Association of Meteorology* (London: Butterworths, 1956); **AMS/RMS** *Proceedings of the Toronto Meteorological Conference* (London: Royal Meteorological Society, 1954)

<sup>32</sup> **R.C. Sutcliffe** ‘Changes of Climate’, *Nature* **4808** (1961): 1139–40.



climatology. At one of the Met Office scientific meetings held in 1961, A.I. Johnson, Lamb's assistant, gave a talk on their ongoing investigations of climate change during the past 200 years. In the next talk, Lamb repeated his claim that, apart from their intrinsic interest, the observational studies of climate change were of real practical value.<sup>34</sup> Strong scepticism was expressed about Lamb's approach in the ensuing discussion. R.G. Veryard, for example, questioned the very usefulness of historical climatology in forecasting future climate trends, and emphasized the superiority of the dynamical approach.<sup>35</sup> Veryard's opinion was commonly shared by other senior meteorologists at the Office. As suggested in Chapter 2, when the British economy was struck by the severe winter of 1962-63 and the political pressure mounted, the Met Office responded by expanding research into dynamical climatology rather than synoptic or historical climatology.

Not all Met Office scientists looked down on the work of Lamb, however. Graham Sutton, director of the Office, was a mathematical physicist specializing in atmospheric turbulence, but was very sympathetic to Lamb's historical research into climate change. It was Sutton who encouraged Lamb to write a new edition of C.E.P. Brooks' *The English Climate*.<sup>36</sup> He also supported Lamb's application for the Royal Meteorological Society prize; when Lamb asked the Office to give him an assistant to plot the pressure maps, Sutton appointed A.I. Johnson to help Lamb's work. In 1963, Sutton promoted Lamb to a special merit scientific officer, allowing him to carry out research without the burden of administrative tasks.<sup>37</sup> On the other hand, for most dynamical meteorologists at the Office, Lamb's historical climatology was not rigorous enough. While Lamb was becoming more and more interdisciplinary in his approach, the general circulation of the atmosphere remained central to his account. Yet his approach was more descriptive than those typically adopted by observational meteorologists and statistical climatologists. This

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33 Ibid.

34 Lamb and Johnson 'Climatic Variation'.

35 Veryard was highly respected by the meteorological community for his discovery in 1961 of the Quasi-Biannual Oscillation (QBO) – a 27-month cyclic reversal of upper winds in the lower stratosphere. QBO was thought to play an important role in the inter-annual climate variability. See R.G. Veryard and R.A. Ebdon 'Fluctuations in Tropical Stratospheric Winds', Ibid. 90 (1961): 125-43.

36 The first edition of 'The English Climate' was published in 1954. Brooks died in 1957. C.E.P. Brooks *The English Climate* (London: English Universities Press, 1954); Goldie 'Obituary - C. E. P. Brooks'.

37 A special merit position was actually awarded to Lamb for his earlier work during the Antarctic expedition, but allowed him to more freely carry out empirical studies of climate change and past climates. See Lamb *Through All the Changing Scenes of Life*; Taba 'Bulletin Interviews - H.H. Lamb'.

certainly did not please senior meteorologists at the Met Office.<sup>38</sup>

The tendency to lay more emphasis on physico-mathematical approaches was further reinforced in 1965 when John Mason replaced Sutton as new director-general of the Met Office. As soon as his appointment was announced, Mason, a renowned cloud physicist, declared that the future of the Office would depend upon the recruitment of high-class physicists and mathematicians.<sup>39</sup> The first thing he did as director-general of the Office was strongly to support numerical weather prediction. He also helped expand the Met.O.20's GCM development. By contrast, Mason was not happy with Lamb's empirical studies.<sup>40</sup> The atmosphere of the Met Office was becoming even more unfavourable to Lamb. When Lamb again asked the Met Office to give him an assistant in order to conduct his research more effectively, he had to face an unsympathetic reaction. By that time, Lamb was ready to publish a paper on the relationship between volcanic eruptions and climate. Having read Lamb's draft, Professor L.R. Wagner, chairman of the British National Volcanological Research Committee, recommended that it be published by the Royal Society. Lamb reported this to his superiors, but they would not allow him to publish. His analysis was considered to be too scientifically vague.<sup>41</sup>

The neglect of the observational studies of past climates continued until the mid to late 1970s when it became clear that they were vital in validating GCMs. This neglect was not just confined to the Met Office but widespread among the British meteorological community. Many academic meteorologists also accepted the view that the proper study of climate and its change could only be realized by physico-mathematical modelling.<sup>42</sup> It was against this background that, in the mid 1960s, the Council of the Royal Meteorological Society decided to downsize its library and to display only the latest

38 Meteorologists at least respected the use of complex statistical methods by climatologists. Lamb's analysis, however, did not involve such methods. **R.S. Harwood** *Interview by the author* (February 25, 1999)

39 **B.J. Mason** 'Some Thoughts on the Future', *Weather* **20** (1965): 273-74. As noted in Chapter 2, he was then a professor of cloud physics at Imperial's meteorology department.

40 **B.J. Mason** *Interview by the author* (July 5, 1999)

41 **Lamb** *Through All the Changing Scenes of Life*; **P.M. Kelly et al.** 'The Contribution of Hubert H. Lamb to the Study of Volcanic Effects on Climate', *Weather* **53** (1998): 209-22. The Met Office allowed Lamb to publish the paper later in 1967 when a small volcanic eruption on Deception Island caused some public attention in Britain. Deception Island was a part of the British Antarctic Territory. The paper was published in 1970. See **Lamb** 'Volcanic Dust in the Atmosphere'.

42 Sheppard of Imperial College worked very hard to initiate a GCM development project in universities. However, due to lack of manpower and financial support, the UK Universities' Atmospheric Modelling Group only started in the 1970s. See **R.C. Sutcliffe and F. Pasquill** 'Percival Albert Sheppard', *Biogr. Mem. Fellows R. Soc.* **25** (1979): 535-53; **R.C. Sutcliffe** 'The Global Atmospheric Research Programme (GARP) and the United Kingdom's Participation', *Q. J. Roy. Meteor. Soc.* **98** (1972): 447-59.

journals and texts. The old meteorological records and the data collected by geologists, palaeobotanists and climatologists did not meet with dynamical meteorologists' approval. Lamb resigned from the Royal Meteorological Society's library committee in protest.<sup>43</sup> In reviewing the evolution of British climatology since the 1960s, one climatologist recalled the pressure that climatologists had to endure, "... as a result of perceived, if not real, pressure from the meteorological community through the research councils, there has been a rush by climatologists to make themselves appear respectable scientists".<sup>44</sup>

### Rise of New Environmental Awareness

During the late 1960s and early 1970s, climate extremes persisted around the world. Despite meteorologists' scepticism, climate variation was increasingly understood as a natural hazard threatening society. This growing recognition that society was vulnerable to climate variations coincided with the upsurge of environmental awareness in the 1960s that human activities were unduly damaging the natural environment. British concern over the environmental degradation was not new to the 1960s – it went back as far as the preservationist movements in the 19<sup>th</sup> century.<sup>45</sup> In 1952, London smog killed more than 4,000 people, shocking the British public and spurring the government into action, and within a few years the Clean Air Act was passed.<sup>46</sup> But environmental concern of the 1960s differed in that it went beyond the controversies over aesthetic countryside or local pollution. It was based on a perception that industrial activities and economic growth were irrevocably depleting and destroying the natural environment.

The emergence of a new environmental awareness was sparked by the publication of Rachel Carson's controversial *Silent Spring* in 1962, which highlighted the possible

43 **Lamb** Interview by R.A.S. Ratcliffe; *Through All the Changing Scenes of Life*.

44 **D.J. Unwin** 'Climatology', in *Quantitative Geography: A British View*, N. Wrigley and R.J. Bennett (eds) (London: Routledge, 1981): 60-69.

45 For example, the Royal Society for the Prevention of Cruelty to Birds was established in 1893. There were also various amenity societies such as the National Trust, which was formed in 1894. See, e.g., **D. Evans** *A History of Nature Conservation in Britain* (London: Routledge, 1997); **J. Sheail** *Nature Conservation in Britain - The Formative Years* (London: Stationery Office, 1998)

46 However, it has been argued by some political scientists that the London Smog of 1952 did not lead to a sudden rise of political activism and was not directly responsible for the passage of the 1956 Clean Air Act. See, e.g., **A. Weale** *The New Politics of Pollution* (Manchester: Manchester University Press, 1992); **J. Sheail** *An Environmental History of Twentieth Century Britain* (London: Palgrave, 2002)

deleterious effects of chemical pollutants.<sup>47</sup> Another event of particular importance in the British context was the Torrey Canyon incident. In 1967, the oil tanker Torrey Canyon spilled 117,000 tonnes of oil in the English Channel, causing massive marine and coastal pollution.<sup>48</sup> As time went on, the scope of concern extended from local and regional pollution problems to those of a more global nature. Membership in preservationist organizations grew substantially, but new, more radical environmentalist groups such as the Conservation Society (1966) and the Friends of the Earth (1971) also entered the scene.<sup>49</sup> The following years witnessed a plethora of new publications warning that the planet earth was at a point of ecological crisis – e.g., *Spaceship Earth* (1966), *The Population Bomb* (1968), *The Doomsday Book* (1970) *A Blue Print for Survival* (1972) and *The Limits to Growth* (1972).<sup>50</sup> John Maddox, an editor of *Nature*, went as far as to refer to these developments as a “doomsday syndrome”.<sup>51</sup>

A sense of impending ecological crisis was further reinforced by the decline of Britain as a world economic and political power. The Suez crisis of 1956-57 strengthened the perception that Britain no longer had the economic strength, the military capability or the political influence to act as a major power. By the late 1960s, in the face of worsening economic conditions, the British became disillusioned with the belief that post-war growth could be sustained.

The British government responded by introducing environment-related agencies. In 1965, in the course of enacting the Science and Technology Bill, the Natural Environment Research Council (NERC) was founded and Graham Sutton, former director of the Met Office, was appointed founder chairman.<sup>52</sup> Four years later, the Central Unit on Environmental Pollution, headed by an ecologist Martin Holdgate, was set up within the

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47 R. Carson *Silent Spring* (Boston: Houghton Mifflin, 1962). The British edition of 'Silent Spring' came out in 1963. As soon as it was published, it resulted in hot debates in the House of Commons over the use of pesticides. See, e.g., **Evans** *A History of Nature Conservation in Britain*; **M. Veldman** *Fantasy, the Bomb, and the Greening of Britain - Romantic Protest, 1945-1980* (Cambridge: Cambridge University Press, 1994).

48 See, e.g., **Sheail** *An Environmental History of Twentieth Century Britain*.

49 See, e.g., **Veldman** *Fantasy, the Bomb, and the Greening of Britain*.

50 **B. Ward** *Spaceship Earth* (New York: Columbia University Press, 1966); **P.R. Ehrlich** *The Population Bomb* (New York: Ballantine Books, 1968); **G.R. Taylor** *The Doomsday Book* (London: Thames & Hudson, 1970); **E. Goldsmith** *A Blueprint for Survival* (Harmondsworth: Penguin, 1972); **D.H. Meadows** (ed) *The Limits to Growth* (London: Earth Island Ltd, 1972)

51 **J. Maddox** *The Doomsday Syndrome* (London: Macmillan, 1972)

52 Originally, the Natural Resources Research Council was proposed as a title. Sutton suggested the substitution of the word 'environment' for 'resources'. **J. Sheail** *Natural Environment Research Council - A History* (Swindon: NERC Publishing Services, 1992)

Cabinet Office.<sup>53</sup> At the same time, the government created the Royal Commission on Environmental Pollution (RCEP), a standing expert advisory body under the chairmanship of the Cambridge botanist Sir Eric Ashby.<sup>54</sup> The Prime Minister Harold Wilson also made a proposal to merge the Ministries of Housing and Local Government, of Transport, and of Public Building Works into the new Department of the Environment, which was implemented by an incoming Conservative government in 1970.<sup>55</sup> The British government did not break with traditional practice, essentially retaining a local and regional perspective and the ethos of science-based, rational planning.<sup>56</sup> Yet its formal acknowledgement of public concern over environmental issues was symbolically important.

The culmination of a new wave of environmental awareness came in 1972 when the United Nations Conference on the Human Environment (UNCHE) was held in Stockholm. First proposed by Sweden, UNCHE was designed to turn the environment into an international political agenda. After four years of preparation, the delegations from 113 countries, 19 intergovernmental agencies, and 400 other intergovernmental and nongovernmental organizations came to Stockholm, resulting in a truly global event.<sup>57</sup> The conferees approved a long-term action plan and recommended setting up a global environmental monitoring system. It also called for a new UN agency that would coordinate the environmental activities of the whole UN system, leading to the formation of UNEP. Perhaps more important than its institutional achievements was the fact that the Stockholm conference epitomized a shift in thinking, at the global level, to the conception of the earth as a vulnerable, interconnected ecosystem, thus lending legitimacy to international environmental action.

This sea change of attitude towards the environment did not have an immediate impact

53 **Central Unit on Environmental Pollution** *The Monitoring of the Environment in the United Kingdom - A Report* (London: HMSO, 1974)

54 See **RCEP** *First Report of the Royal Commission on Environmental Pollution* (London: HMSO, 1971). For the role of RCEP in British environmental politics, see **S. Owens and T. Rayner** "When Knowledge Matters" - The Role and Influence of the Royal Commission on Environmental Pollution', *J. Environ. Pol. Plann.* 1 (1999): 7-24.

55 **P. Draper** *Creation of the D.O.E.*, Civil Service Studies, No. 4 (London: HMSO, 1977)

56 Lowe and Flynn argue that the British government's approach prior to the Thatcher era was "informal, accommodative and technocratic". See **P. Lowe and A. Flynn** 'Environmental Politics and Policy in the 1980s', in *The Political Geography of Contemporary Britain*, J. Mohan (ed) (Basingstoke: Macmillan, 1989): 255-79. For general discussion on British environmental policy, see, e.g., **P. Lowe** *Environmental Groups in Politics* (London: George Allen & Unwin, 1983); **Weale** *The New Politics of Pollution*; **P. MacNaghten and J. Urry** *Contested Natures* (London: Sage, 1998).

57 **W. Kennel** 'The Stockholm Conference on the Human Environment', *Int. Aff.* 48 (1972): 33-45.



on the way in which British climate research was organized and practised.<sup>58</sup> It nevertheless had important consequences. The static view of climate came to be challenged and, along with natural climate variability, anthropogenic climate change – or climate change as a pollution problem – began to be more widely debated both in public and scientific communities. The climate chapter of G.R. Taylor's *The Doomsday Book*, for instance, devoted most of its space to human impact on climate.<sup>59</sup> In 1969, the WMO Commission for Climatology re-established its Working Group on Climatic Fluctuations to review anthropogenic as well as natural climate variations.<sup>60</sup> The WMO also commenced the Background Air Pollution Monitoring Network (BAPMoN) to observe changes in the chemical composition of the atmosphere. According to D.A. Davies, secretary-general of WMO, one of the BAPMoN's two main goals was to investigate "whether the pollutants interfere to a significant extent with natural atmospheric processes and, if so, whether such interference is having long-term effects on the earth's climate".<sup>61</sup> Likewise, the Stockholm action plan suggested that observing stations be built in areas remote from sources of pollutions "in order to monitor long-term global trends in atmospheric constituents and properties which may cause changes in meteorological properties, including climate changes" and that, if necessary, the WMO and ICSU launch new programmes "to understand the general circulation of the atmosphere and the causes of climatic changes whether these causes are natural or the result of man's activities".<sup>62</sup>

### Human Impact on Climate: Early Discussions

What first drew the attention of some of the mainstream meteorologists to the topic of anthropogenic climate change, though, were the early controversies surrounding large-scale weather and climate modification.

Responding to public outcry over the potential hazards of fallout from atmospheric

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58 For example, the British government's response to the UNCHE was rather modest. See **Department of the Environment** *The Human Environment - The British View* (London: HMSO, 1972)

59 **Taylor** *The Doomsday Book* .

60 **WMO Commission for Climatology** *Abridged Final Report - Fifth Session* (Geneva: WMO, 1969) In 1971, the WMO Commission for Climatology was renamed the Commission for Special Applications of Meteorology and Climatology.

61 **D.A. Davies** 'The Role of the WMO in Environmental Issues', *Int. Organ.* **26** (1972): 327-36.

62 See Recommendation 79 of the UNCHE Action Plan. **UNCHE** *Action Plan for the Human Environment* (Nairobi: UNEP, 1972)

nuclear tests, the US Congress Joint Committee on Atomic Energy held a series of hearings in the late 1950s and summoned a number of scientists to testify.<sup>63</sup> While the focus of hearings was primarily on the biological and health effects of radioactive fallout, meteorologists from the US Weather Bureau and other institutions were also invited to look into the reciprocal effects of fallout and meteorological conditions.<sup>64</sup> In 1956, Eisenhower's Advisory Committee on Weather Control set up the Panel on Meteorological Effects of Nuclear Explosions, which included John von Neumann, a father of numerical weather prediction.<sup>65</sup> The findings of these meteorologists were not so frightening<sup>66</sup>; they concluded that fallout had only local and limited effects on the weather. They nonetheless pointed out that humankind might be able to modify climate, through a change in the earth's albedo, by a significant amount of dust particles.

At the time in the US, the issue of large-scale weather and climate modification was often identified as a security problem. The Cold War was at its zenith, and the risk of nuclear holocaust was very real. Furthermore, since the mid 1950s, Russian scientists had put forward numerous climate modification schemes to warm the northern part of the country using hydrological means – e.g., by building a dam across the Bering Strait between Siberia and Alaska and pumping cold Arctic water southward into the Pacific.<sup>67</sup> These ideas were considered by some to be potential threats to the US. Henry G. Houghton, a distinguished meteorologist and Chair of MIT's Meteorology Department, said, "An unfavorable modification of our climate in guise of a peaceful effort to improve Russia's climate could seriously weaken our economy and ability to resist".<sup>68</sup> Many

63 **US Congress Joint Committee on Atomic Energy** *The Nature of Radioactive Fallout and Its Effects on Man - Hearings before the Special Subcommittee on Radiation (85<sup>th</sup> Congress)* (Washington, DC: US GPO, 1957)

64 By the early 1950s, the AEC, the Department of Defense and the RAND Corporation all had their own fallout research programmes.

65 **C. Kwa** 'The Rise and Fall of Weather Modification - Changes in American Attitudes Towards Technology, Nature, and Society', in *Changing the Atmosphere - Expert Knowledge and Environmental Governance*, C.A. Miller and P.N. Edwards (eds) (Cambridge: MIT Press, 2001): 135-65; **US Advisory Committee on Weather Control** *Final Report* (Washington, DC: US GPO, 1958)

66 Some meteorologists thought that atmospheric nuclear tests provided a unique research opportunity. L. Matcha and D.L. Harris of NCAR wrote in 1961, "For centuries meteorologists have thought of exploring large-scale atmospheric circulations by means of tracers. ... Although it is not proposed that special nuclear tests be undertaken for meteorological purposes, it seems reasonable to expect even greater value from future tests using an expanded network and having detonations at other locations and times." See **L. Machta and D.L. Harris** 'Effects of Atomic Explosions on Weather', *Science* **121** (1955): 75-81.

67 See, e.g., **D.W. Keith** 'Geoengineering the Climate - History and Prospect', *Annu. Rev. Energy. Env.* **25** (2000): 245-84; **S.R. Weart** 'Climate Modification Schemes', in *The Discovery of Global Warming*, <http://www.aip.org/history/climate> (recorded copy at American Institute of Physics, 2001): p. RainMaking-3 of 08/01/01 text.

68 **US Congress House Committee on Interstate and Foreign Commerce** *Weather Modification Research -*

meteorologists were dubious about such possibilities, but the investigations into this area were generously supported.

In Britain, too, the Met Office scientists were called on to give advice concerning nuclear fallout. Their verdict was similar to that of Americans. Sutton, director of the Office, argued in 1955 that nuclear tests could not be held responsible for any world-wide weather anomalies.<sup>69</sup> Three years later, after revisiting the question, he reached the same conclusion.<sup>70</sup> However, this scepticism was even stronger and more widespread among British meteorologists than amongst their American counterparts. Although British meteorologists were well aware of the crucial role of albedo in determining the earth's radiation budget, they tended to think that its alteration by humans would be very unlikely. Public anxiety rose as the Windscale reactor, designed to produce plutonium for nuclear weapons, caught fire in 1957 and released massive amounts of radioactive substances into the atmosphere.<sup>71</sup> Yet, apart from Sutton's comments, no formal discussion took place on the effects of fallout on large-scale weather and climate conditions. The Met Office's Special Investigations Branch confined its analysis chiefly to the atmospheric dispersion of nuclear fallout.<sup>72</sup>

Russian climate modification schemes were no exception. Throughout the 1950s and early 1960s, they passed almost unnoticed by British meteorologists. The subject regained some media attention in the mid 1960s when it became known that, in order to remove the Arctic sea ice cover, Russia was planning to canalize some of the great rivers in Siberia away from the Arctic Ocean.<sup>73</sup> In the US, mainly due to concern over the unintended climatic consequences of the Russian plan, Joseph O. Fletcher of the RAND Corporation initiated a research programme on the Arctic heat budget and its relation to climate.<sup>74</sup> The Met Office scientists did not instigate equivalent research efforts, partly because the most affected area would be the Northern Pacific, an area of little interest to the UK, but also

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*Hearings before the Subcommittee (85<sup>th</sup> Congress)* (Washington, DC: US GPO, 1958)

69 **G. Sutton** 'Thermonuclear Explosions and the Weather', *Nature* **175** (1955): 319-21.

70 **G. Sutton** 'Man's Attempts to Control the Weather', *New Sci.* (September 4, 1958): 744-46.

71 **L. Arnold** *Windscale 1957 - Anatomy of a Nuclear Accident* (London: Macmillan, 1992)

72 See, e.g., **J. Crabtree** 'The Travel and Diffusion of the Radioactive Material Emitted during the Windscale Accident', *Q. J. Roy. Meteor. Soc.* **85** (1959): 362-70; **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1957-59)

73 **Keith** 'Geoengineering the Climate'; **Weart** 'Climate Modification Schemes'. See also **L. Ponte** *The Cooling* (Englewood Cliffs: Prentice-Hall, 1976)

74 See, e.g., **J.O. Fletcher** *The Heat Budget of the Arctic Basin and Its Relation to Climate*, RAND Paper R-

because they were reluctant to engage in any debates that might cause alarm. More fundamentally, the prospect of humans modifying the weather or climate on a large scale, either deliberately or inadvertently, was still regarded as a distant possibility. In 1966, Reginald Sutcliffe, now at the University of Reading, complained that in Britain weather and climate modification “has barely crossed the threshold of scepticism to be fathered by respectable scientists”.<sup>75</sup>

Somewhat paradoxically, it was Hubert Lamb, a strong proponent of the observational studies of natural climate variations, who took up the issue of anthropogenic climate change more seriously. As noted, Lamb reviewed a range of possible causes of climate variation in the late 1950s, and concluded that anthropogenic causes did not play a significant role. By the mid 1960s, however, he began to think that human impact on climate, including the Russian river diversion plan, could no longer be ignored. He wrote in the preface of his 1966 book *The Changing Climate*:

[I]t begins to appear possible that Man might himself be able to modify world climates either by deliberate contrivance or as an accidental by-product of his other activities. There are clearly hazards involved in any grandiose scheme to change the world distribution of climate significantly – for instance, by such costly and technically difficult operations as removal of the Arctic sea-ice.<sup>76</sup>

Lamb was as cautious as other Met Office scientists, and emphasized that further research should be undertaken before any firm conclusions about its repercussions could be drawn. Nonetheless, he went on to warn that undesirable side effects might arise from such a scheme – “a climate emergency” as he called it.<sup>77</sup>

### Effects of Atmospheric Pollution on Global Climate

Meanwhile, outside Britain, interest in the effects of pollution on the global atmosphere continued to grow. This, coupled with Charles D. Keeling’s reports in the early 1960s that he was able to demonstrate a steady rise in the atmospheric CO<sub>2</sub> level from his monitoring

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0444-PR (Santa Monica: RAND Corporation, 1965)

75 **R.C. Sutcliffe** ‘Control of Weather and Climate’, *Nature* **210** (1966): 459-60.

76 **H.H. Lamb** *The Changing Climate - Selected Papers* (London: Methuen, 1966), at ix.

77 **H.H. Lamb** ‘Climate Engineering Schemes to Meet a Climatic Emergency’, *Earth-Sci. Rev.* **7** (1971): 87-95.

work at the Mauna Loa Observatory,<sup>78</sup> led to the re-examination of the earlier discussions of anthropogenic climate change by Guy Callendar, Gilbert Plass, Roger Revelle and Hans Suess.<sup>79</sup>

In 1965, the Environmental Pollution Panel of the US President's Science Advisory Committee (PSAC) issued its official report *Restoring the Quality of Our Environment*, which had a chapter on the climatic effects of increased atmospheric CO<sub>2</sub>.<sup>80</sup> Around that time, Syukuro Manabe of GFDL was asked by the NAS Panel on Weather and Climate Modification to examine the dependence of the earth's surface temperature on the atmospheric CO<sub>2</sub> content.<sup>81</sup> By using a 1-D radiative-transfer model with a convective adjustment – or as it was usually called a radiative-convective model (RCM) – devised in their GCM development, Manabe and his co-worker Richard Wetherald estimated that a doubling CO<sub>2</sub>, with a fixed absolute humidity assumption, would increase the mean surface temperature by *ca.* 1 °C. Later in 1967, when they used a given distribution of relative humidity, the figure doubled to *ca.* 2 °C.<sup>82</sup> In Russia, based upon the results of his simple energy balance model (EBM), Mikhail Budyko insisted that the increase of atmospheric CO<sub>2</sub> content might cause the Arctic ice to disappear.<sup>83</sup> William D. Sellers at the University of Arizona-Tucson, who followed Budyko's approach but developed his own 1-D EBM, came to a similar conclusion.<sup>84</sup>

Anthropogenic factors other than CO<sub>2</sub> also began to be considered. Manabe's model calculations mentioned above included a five-fold increase of stratospheric water vapour. These results were used in assessing the effects of water vapour exhausted by supersonic

78 See, e.g., **C.D. Keeling** 'The Concentration and Isotopic Abundances of Carbon Dioxide in the Atmosphere', *Tellus* **12** (1960): 200-03; 'The Concentration and Isotopic Abundances of Carbon Dioxide in Rural and Marine Air', *Geochim. Cosmochim. Ac.* **24** (1961): 277-98.

79 For the work of Callendar, Plass, Revelle and Suess, see, e.g., **G.S. Callendar** 'The Artificial Production of Carbon Dioxide and Its Influence on Temperature', *Q. J. Roy. Meteor. Soc.* **64** (1938): 223-37; **G.N. Plass** 'The Carbon Dioxide Theory of Climatic Change', *Tellus* **8** (1956): 140-54; **R. Revelle and H.E. Suess** 'Carbon Dioxide Exchange between Atmospheric and Ocean and the Question of an Increase of Atmospheric CO<sub>2</sub> during the Past Decades', *Tellus* **9** (1957): 18-27. See also Chapter 1 and 4.

80 **R. Revelle et al.** 'Carbon Dioxide from Fossil Fuels - The Invisible Pollutant', in *Restoring the Quality of Our Environment* (Washington, DC: The White House, 1965): 111-33. See also Chapter 4.

81 **US NRC** *Weather and Climate Modification - Problems and Prospects, Vol. 2: Research and Development* (Washington, DC: National Academy of Sciences, 1966). See also Chapter 4.

82 **S. Manabe and R.T. Wetherald** 'Thermal Equilibrium of Atmosphere with a Given Distribution of Relative Humidity', *J. Atmos. Sci.* **24** (1967): 241-59.

83 **M.I. Budyko** 'Polar Ice and Climate', in *Proceedings - Symposium on the Arctic Heat Budget and Atmospheric Circulation*, J.O. Fletcher (ed) (Santa Monica: RAND Corporation, 1966): 3-21; 'Effect of Solar Radiation Variations on Climate of Earth', *Tellus* **21** (1969): 611-19.

84 **W.D. Sellers** 'A Global Climate Model Based on the Energy Balance of the Earth-Atmosphere System', *J. Appl. Meteorol.* **8** (1969): 392-400.



transports (SSTs) on climate. Public and scientific concern about the environmental effects of SSTs shifted in 1970 to the potential destruction of stratospheric ozone by nitrogen oxides (NO<sub>x</sub>) emissions, resulting in a fierce controversy.<sup>85</sup> Some scientists proposed yet another anthropogenic cause of climate change. They claimed that, as a result of human industrial and agricultural activities, atmospheric aerosols were building up, thereby increasing the earth's albedo.<sup>86</sup> Reid Bryson, a meteorologist at the University of Wisconsin-Madison, argued that this, more than volcanic eruptions, could outweigh the warming effect of CO<sub>2</sub> and ultimately decrease the earth's temperature. In 1971, S. Ichtiaque Rasool and Stephen H. Schneider of the NASA Goddard Institute of Space Studies (GISS) compared the effects on global climate of large increases in atmospheric CO<sub>2</sub> and aerosols, utilizing a different 1-D radiative-transfer model developed for studies of other planetary atmospheres. While Rasool and Schneider were concerned with the bigger issue of human impact on climate, their study, like Manabe's work, was basically a climate sensitivity experiment and did not attempt to "predict" the future climate.<sup>87</sup> But they contended that the projected rate of increasing aerosol injection could lead to a decrease in global temperature by *ca.* 3.5 °C, which "is believed to be sufficient to trigger an ice age", and that, even with the effects of doubling CO<sub>2</sub> considered, the net result could be a "cooling of Earth".<sup>88</sup>

Within a very short period, anthropogenic climate change had been transformed from a subject that concerned only a few research groups to a potentially far-reaching environmental threat. Both Manabe and Bryson presented their results in 1968 at the American Association for the Advancement of Science (AAAS) symposium on Global Effects of Environmental Pollution, which not only scientists such as Revelle but also scientist-activists such as Barry Commoner attended.<sup>89</sup> More significant in framing

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85 **V.D. Nuessle and R.W. Holcomb** 'Will the SST Pollute the Stratosphere?' *Science* **168** (1970): 1562. See also **F. Drake and M. Purvis** 'The Effect of Supersonic Transports on the Global Environment - A Debate Revisited', *Sci. Technol. Hum. Val.* **26** (2001): 501-28.

86 **R.A. Bryson** "All Other Factors Being Constant ... " A Reconciliation of Several Theories of Climatic Change', *Weatherwise* **21** (1968): 56-61.

87 *Climate sensitivity* refers to the steady-state increase in the global annual mean surface temperature associated with a given global-mean radiative forcing.

88 **S.I. Rasool and S.H. Schneider** 'Atmospheric Carbon Dioxide and Aerosols - Effects of Large Increases on Global Climate', *Science* **173** (1971): 138-41. Stephen Schneider received his PhD in Mechanical Engineering and Plasma Physics from Columbia University. Upon completing his PhD, he studied the role of greenhouse gases and suspended particulate material on climate as a postdoctoral fellow at NASA GISS. **S.H. Schneider** *Interview by the author* (January 23, 2001). See also Chapter 4.

89 **S.F. Singer** (ed) *Global Effects of Environmental Pollution* (Dordrecht: D. Reidel, 1970) It was ironic that the organiser of the AAAS symposium was S. Fred Singer, who later became one of the most active anti-global

anthropogenic climate change as a global environmental problem were the two workshops held in 1970 and 1971 – the Study of Critical Environmental Problems (SCEP) and the Study of Man's Impact on Climate (SMIC).<sup>90</sup> The SCEP's Work Group on Climatic Effects, chaired by William W. Kellogg of NCAR, reviewed the state of knowledge on possible human-induced causes of climate change, from atmospheric CO<sub>2</sub>, aerosols, land surface changes, to SSTs. Having been organized more focusedly, SMIC extended SCEP's discussions and affirmed that humankind could already be influencing the climate on a global scale.

SMIC was also an American initiative, but was more international than SCEP and held in Wijk, Sweden. Some of the leading foreign scientists in the field – *e.g.*, Budyko from Russia and Bert Bolin from Sweden – were invited.<sup>91</sup> It was unusual that there was no British participant except George D. Robinson, who, together with William W. Kellogg, served as joint secretaries.<sup>92</sup> To a large degree, this reflected the status of British climate research in the early 1970s. There were few climatologists who were interested in anthropogenic climate change.<sup>93</sup> Many dynamical meteorologists were indifferent to the very subject of climate and its change. The Met.O.20 had only a handful of scientists, who were preoccupied with developing their first GCM, and was yet to expand its manpower and computing capabilities.<sup>94</sup> John Houghton's group at Oxford, who otherwise had knowledge and skills to perform radiative-transfer calculations for the earth's atmosphere, was busy working on remote sounding from satellites.<sup>95</sup> But there was another reason as

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warming scientists. For Manabe and Bryson's contributions, see **S. Manabe** 'The Dependence of Atmospheric Temperature on the Concentration of Carbon Dioxide': 25-29 and **R.A. Bryson and W.M. Wendland** 'Climate Effects of Atmospheric Pollution': 130-138.

90 **SCEP** *Man's Impact on the Global Environment - Report of the Study of Critical Environmental Problems* (Cambridge: MIT Press, 1970); **SMIC** *Inadvertent Climate Modification - Report of the Study of Man's Impact on Climate* (Cambridge: MIT Press, 1971)

91 SMIC brought together 35 climate scientists from 15 countries. See **SMIC** *Inadvertent Climate Modification*. Bert Bolin was director of the International Meteorological Institute at the University of Stockholm. For Bolin's brief biography, see Chapter 4, note 16.

92 As mentioned in the previous chapter, G.D. Robinson had worked as an atmospheric physicist at the Met Office. After retiring from the Office, he emigrated to the US to join Tom Malone's Travelers' Research Corporation in 1968. **G.D. Robinson** *Interview by E. Droessler* (AMS Tape Recorded Interview Project: June 27-28, 1994)

93 G.S. Callendar had died by that time. **Anon.** 'Obituary - G. S. Callendar', *Q. J. Roy. Meteor. Soc.* **91** (1965): 112. There were a few scientists from other disciplines who showed interest in the CO<sub>2</sub>-climate connection, but they did not have much contact with the meteorological or climatological community.

94 Andrew Gilchrist, one of the Met.O.20's founding members, suggested that GFDL's GCM development was greatly assisted by Japanese scientists such as Manabe and Myakoda. In the post war reconstruction period, Britain could not afford to employ foreign scientists. **A. Gilchrist** *Interview by the author* (June 9, 1999)

95 **J.T. Houghton and S.D. Smith** 'Remote Sounding of Atmospheric Temperature from Satellites - 1. Introduction', *Proc. R. Soc. Lon. Ser.-A* **320** (1970): 23-32.

well. Hart and Victor suggest that SMIC was carefully cultivated to link science and environmental politics; its final report was prepared as a kind of background paper for the Stockholm Conference.<sup>96</sup> For many British meteorologists, SMIC did not carry the authority to convey an essentially political message on behalf of the scientific community. In his review of the SMIC report, Sutcliffe rather cynically wrote:

[It] read like the report of a Royal Commission except they are addressed to no responsible executive body. 'We recommend ...' appears so often in the text, perhaps a hundred times, perhaps several hundred, that we at once ask to whom the advice is tendered ...<sup>97</sup>

That alone could have made the Met Office reluctant to send its staff to the workshop.

This does not mean that the issue of human impact on climate was simply ignored in Britain. In February 1971, before SMIC was held, the RCEP published its first report.<sup>98</sup> It had a short chapter on anthropogenic climate change, summarizing the existing knowledge about the effects on weather and climate of atmospheric CO<sub>2</sub>, dust, water vapour from SSTs, rocket exhaust chemicals, land use and waste energy. Scientifically, its appraisal was not much different from those of SCEP and SMIC. But the report was written in a more guarded tone, emphasizing uncertainty and lack of understanding. Also, even though the RCEP considered it important for Britain to carry on research in the field, it did not recommend any significant changes in the way British climate research had been carried out. The RCEP did not have meteorologist or climatologist staff, and the chapter was written in consultation with the Met Office.<sup>99</sup> So the viewpoints represented in that chapter mirrored those of the Met Office.

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96 **D.M. Hart and D.G. Victor** 'Scientific Elites and the Making of US Policy for Climate Change Research, 1957-74', *Soc. Stud. Sci.* **23** (1993): 643-80. It is interesting to note that Carroll Wilson, an MIT professor of management and the main organizer of SCEP and SMIC, was a member of the Club of Rome, which sponsored 'The Limits to Growth' study in 1972. See **P.N. Edwards** 'The World in a Machine - Origins and Impacts of Early Computerized Global Systems Models', in *Systems, Experts, and Computers*, T.P. Hughes and A.C. Hughes (eds) (Cambridge: MIT Press, 2000)

97 **R.C. Sutcliffe** 'Book Review - *Inadvertent Climate Modification: Report of the Study of Man's Impact on Climate*', *Q. J. Roy. Meteor. Soc.* **98** (1972): 708-09.

98 **RCEP** *First Report of the Royal Commission*.

99 **Mason** *Interview by the author*.

## General Circulation Modelling as a Principal Methodology

Again, it was Hubert Lamb who vocally underscored the need for more research into anthropogenic climate change. In fact, Lamb believed that anthropogenic factors were less important in controlling climate than natural factors such as volcanic eruptions or atmosphere-ocean interaction. But he thought that they did have an impact, and needed to be scrutinized. Moreover, his descriptive and historical approach did not require discriminating anthropogenic from natural, and external from internal forcings. His programme comprised two related empirical studies: one that would reconstruct the past behaviour of the natural climate in times when it must have been unaffected by human modification of the environment; and another based on the data obtained by monitoring the variations of climate over the recent industrial period.<sup>100</sup> Observation was the key word, and there was no qualitative difference between the two. It was not that straightforward for the modelling approach, in which more sophisticated tinkering was required to link thermodynamics (external forcings; *e.g.*, radiation) to dynamics (internal forcings; *e.g.*, atmospheric motion). Whether one concentrated on the effects of atmospheric CO<sub>2</sub> or natural climate variability could make a difference.

Lamb's approach was severely criticized by dynamical meteorologists. In 1970, Lamb gave a talk on climate variation to the Royal Meteorological Society meeting. He listed a number of causes of climate change, both natural and anthropogenic, and asserted that improvement of knowledge and understanding would depend on: (i) isolating and assessing the nature and magnitude of the effects of each of these influences separately; (ii) interpreting past climate variations in terms of the probable contributions of each factor; and (iii) monitoring current climate changes. John Mason responded quite dismissively:

Questions are now being asked ... about the possibility of modifying weather and climate inadvertently ... [W]e are still a very long way from being able either to detect or predict their effects on world-wide weather and climate. ... The atmosphere is such a complex system ... that it is scarcely possible to distinguish between cause and effect and therefore intuitive and qualitative arguments are of little value. Progress is likely to be made only by the construction of sophisticated physico-mathematical models of the

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100 H.H. Lamb 'The New Look of Climatology', *Nature* **223** (1969): 1209-15.

global atmosphere incorporating all the major physical processes and to use these to simulate and predict the highly non-linear behaviour of the system as a whole. ... At this stage the requirement, as I see it, is for sensible, well-conceived programmes of monitoring and measurement that will provide data for the calculations when these become possible. Meanwhile, we will do well as a profession to replace much of the present emotion by some hard facts and figures.<sup>101</sup>

Mason admitted that enhanced monitoring and measurement were necessary, but only to be fed into models. Lamb attempted to defend his position:

It is not difficult to point to cases where valuable progress has been made in our ability to understand and predict climatic behaviour by much simpler means than the elaborate mathematical models (for all their value in attempting to explore non-linear and secondary consequences, arising from reactions to any primary disturbance) of the atmospheric system. In many case [*sic*] of the primary cause-effect relationship of atmospheric behaviour in the presence or absence of some external disturbance A is so strong that to recognize it is important. ... It is vital that we should study the matter independently, and that our study ... should not be confined to theoretical exploration with mathematical simulation modelling, but should also proceed empirically, *i.e.* by observing what does happen and establishing what did happen in other relevant circumstances in the past.<sup>102</sup>

His reply was not satisfactory for Mason and other senior meteorologists at the Met Office. The static conception of climate was no longer accepted. They nevertheless believed that physico-mathematical approaches were the only reliable way of tackling climate-related problems. Climate was viewed as an average atmospheric state that was in equilibrium with slowly changing external boundary conditions. For fixed or only slowly changing external forcings, if integrated through an extended period of simulated time, it was presumed that a numerical solution (*i.e.*, an equilibrium climate) could be derived from GCM calculations. Noticeable climate change would occur when the effects of changes in external forcings (either natural or man-made) were large enough to outweigh

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101 From **H.H. Lamb** 'Climatic Variation and Our Environment Today and in the Coming Years', *Weather* **25** (1970): 447-55. This was not just Mason's personal view. As early as in 1966, the Met Office's Annual Report stated, "The importance of quantitative studies of the general circulation of the atmosphere has been underlined during the year by the continued scientific interest shown internationally in the possibility of large-scale weather modification by human action, deliberate or accidental." See **British Met Office Annual Report of the Meteorological Office** (London: HMSO, 1966)

102 **Lamb** 'Climatic Variation and Our Environment Today' .



the internal fluctuations within climate. Even then, it was thought, a thorough examination of atmospheric dynamics and thermodynamics would be necessary, something which Lamb did not provide in his approach.

The senior scientists at the Met Office were not just critical of Lamb's empirical approach. They were every bit as disapproving of simple climate modelling that did not explicitly deal with atmospheric dynamics. The development and use of GCMs was, without a doubt, embraced by dynamical meteorologists elsewhere. This was the main theme of the "Global Circulation of the Atmosphere" conference jointly held by the Royal, American and Canadian Meteorological Societies in 1969.<sup>103</sup> The final reports of SCEP and SMIC, among other things, recommended the expansion of GCM developments.<sup>104</sup> In October 1970, British universities also initiated their own, more theoretically oriented, GCM project. Funded by NERC, the UK Universities' Atmospheric Modelling Group was based at the University of Reading, with participation of dynamical meteorologists from Imperial, Exeter, Oxford and Cambridge.<sup>105</sup>

Because GCMs were in a primitive stage of development, however, many scientists abroad were willing to use simple models for their climate studies. Much of the modelling work in the SMIC report was based on simple climate models,<sup>106</sup> as were many other climate sensitivity studies carried out during the late 1960s and 1970s. The 1-D, horizontally averaged radiative calculations by Manabe and Wetherald and by Rasool and Schneider were designed to examine the relative contribution of various atmospheric constituents for the radiation balance and temperature distribution of the earth-atmosphere system.<sup>107</sup> In 1969, as noted above, Budyko and Sellers simultaneously developed 1-D,

103 **G.A. Corby** (ed) *The Global Circulation of the Atmosphere* (London: Royal Meteorological Society, 1969)

104 **SCEP** *Man's Impact on the Global Environment*; **SMIC** *Inadvertent Climate Modification*.

105 It was Sheppard who first made a suggestion to the NERC to fund an inter-universities modelling group. He naturally wanted to establish it at Imperial. As John Green declined to lead the group, however, the universities group had to be based at the University of Reading's Meteorology department led by R.P. Pearce. Initially, the group used a basic, 5-level grid point GCM, but then decided to develop a spectral GCM. **J.S.A. Green** *Interview by the author* (August 3, 1999). See also **R.P. Pearce and B.J. Hoskins** 'Advances in Atmospheric Circulation Studies', *NERC News* 1 (1987): 5-9; **Sutcliffe** 'The Global Atmospheric Research Programme (GARP) and the United Kingdom's Participation'.

106 By the time the SMIC was held, Manabe actually had the GCM results for a CO<sub>2</sub>-doubling experiment. Scientists within the modelling community were aware of his early GCM results. However, Manabe did not present or publish them until the mid 1970s. Manabe first presented his GCM results at the GARP Study Conference held in 1974. See **Schneider** *Interview by the author*, **S. Manabe** 'The Use of Comprehensive General Circulation Modelling for Studies of the Climate and Climate Variation', in *The Physical Basis of Climate and Climate Modelling* (Geneva: WMO, 1975): 148-62.

107 **Manabe and Wetherald** 'Thermal Equilibrium of Atmosphere'; **Rasool and Schneider** 'Atmospheric Carbon Dioxide and Aerosols'.

zonally and vertically averaged EBMs.<sup>108</sup> Their models were even simpler than radiative-transfer models, but permitted a greater sensitivity to small changes by introducing the feedback effect of surface temperature-ice-albedo coupling. Sellers later improved his EBM to include 2-D horizontal resolution.<sup>109</sup> Although atmospheric dynamics was included only in a crude parameterized form, those who developed and used these simple models still considered them as useful tools for studying the stability and sensitivity of the earth's climate.

This did not persuade the Met Office scientists to use 1-D or 2-D models in climate research. Budyko-Sellers models were seen as too simple, even by academic meteorologists who themselves had difficulty in acquiring computing power and time. Often dubbed “toy models”, they were taken as something belonging to geographers.<sup>110</sup> Radiative-transfer models were treated somewhat differently, since they were viewed as involving more rigorous mathematical techniques and comprehensive knowledge of atmospheric physics. The 1967 work of Manabe and Wetherald at GFDL, in particular, was highly regarded.<sup>111</sup> For the Met Office scientists, though, it was important that this was an integral part of GCM developments. Whereas Manabe and Wetherald did use a 1-D RCM to study the climatic effects of increased CO<sub>2</sub>, their major research goal was the formulation of a radiative transfer scheme for GFDL's GCM. The NAS request to assess the greenhouse effect was secondary to this goal.<sup>112</sup> The Met Office scientists' respect for their results also had less to do with the CO<sub>2</sub>-doubling experiment. By then, Clive Rodgers and Charles D. Walshaw at Oxford had developed a similar radiative transfer scheme for the Met.O.20 GCM. Not surprisingly, it was not understood and used as a simple climate model.<sup>113</sup>

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108 **Sellers** 'A Global Climate Model Based on the Energy Balance'; **Budyko** 'Effect of Solar Radiation Variations on Climate of Earth'.

109 **W.D. Sellers** 'A New Global Climatic Model', *J. Appl. Meteorol.* **12** (1973): 241-54; '2-Dimensional Global Climatic Model', *Mon. Weather Rev.* **104** (1976): 233-48.

110 **Harwood** *Interview by the author*; **Green** *Interview by the author*.

111 **Harwood** *Interview by the author*.

112 Since then, however, it has become common practice to use CO<sub>2</sub>-doubling as a benchmark for comparing GCM climate sensitivities. Consequently, climate sensitivity is often defined narrowly as the change in global-mean temperature that would be reached following a doubling of atmospheric CO<sub>2</sub> content.

113 The choice of the radiative transfer scheme was dictated by what kind of GCM was to be constructed. As suggested in Chapter 2, the Met Office's priority was given to the development of GCM that could simulate the presently observed climate without using too much computing time. There was no particular reason why Rodgers and Walshaw should use a CO<sub>2</sub>-doubling condition. For the Rodgers-Walshaw radiation scheme, see **C.D. Rodgers and C.D. Walshaw** 'Computation of Infra-Red Cooling Rate in Planetary Atmospheres', *Q. J. Roy. Meteor. Soc.* **92** (1966): 67-92.

There was another type of simple climate model, which was more in tune with dynamical meteorologist's thinking. In 1970, John Green at Imperial College, Eric Eady's former student, proposed an alternative approach to investigate short-term climate variations. He built a 2-D statistical-dynamical model (SDM) that would numerically solve the basic dynamics, but would only deal with the global features of the time-averaged wind and temperature fields without directly computing the transport properties of smaller-scale atmospheric motion.<sup>114</sup> While relatively simple and low-resolution, this model allowed a more realistic representation of atmospheric dynamics than 1-D models; Green's results seemed to describe some aspects of global-scale atmospheric transports quite realistically.<sup>115</sup> In the US, Peter H. Stone at MIT spearheaded efforts to develop a 2-D SDM, and several other research groups followed suit.<sup>116</sup> The Met Office scientists acknowledged that this approach might help circumvent the vast amount of computation required by GCMs.<sup>117</sup> Despite this, the Met.O.20 continued to insist on the centrality of GCMs, and this line of research was not pursued.

### Early Climate Modelling Work at the Met Office

The Met Office's early motivation for setting up the Met.O.20, as noted in Chapter 2, was closely tied to the development of dynamical long-range weather forecasting. However, the prospect of dynamical long-range forecasting was already being called into question during the mid to late 1960s. Tests by US GCM research groups including GFDL, UCLA and NCAR indicated that the limit of deterministic atmospheric predictability was about 2 weeks.<sup>118</sup> In 1967, George D. Robinson, president of the Royal Meteorological Society,

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114 **J.S.A. Green** 'Transfer Properties of the Large-scale Eddies and the General Circulation of the Atmosphere', *Ibid.* **96** (1970): 157-85.

115 *Ibid.*

116 **P.H. Stone** 'Effect of Large-scale Eddies on Climatic Change', *J. Atmos. Sci.* **30** (1973): 521-29. For a review of subsequent SDM works, see **B. Saltzman** 'A Survey of Statistical-Dynamical Models of Terrestrial Climate', *Adv. Geophys.* **20** (1978): 183-304.

117 **B.J. Mason** 'Towards the Understanding and Prediction of Climatic Variations', *Q. J. Roy. Meteor. Soc.* **102** (1976): 473-98.

118 **E.N. Lorenz** 'The Future of Weather Forecasting', *New Sci.* (May 8, 1969): 290-91; 'Three Approaches to Atmospheric Predictability', *B. Am. Meteorol. Soc.* **50** (1969): 345-49; **A.L. Hammond** 'Global Meteorology (II) - Numerical Models of the Atmosphere', *Science* **174** (1971): 393-95. In 1963, E.N. Lorenz, a dynamical meteorologist at MIT, discovered that small differences in the basic atmospheric state used to initialize a computer model for weather prognostication could lead to divergent solutions as the model integrated forward in time. A year later, Lorenz and other meteorologists involved in GCM development gathered at NCAR to investigate the outer limits of forecast potential. Participants included Jule Charney (MIT), Joseph Smagorinsky (GFDL), Yale Mintz (UCLA) and Cecil Leith (NCAR).

raised the same issue in his presidential address, discomfiting scientists from the Met Office.<sup>119</sup> This realization, at least partly, led the Met Office to engage more in exploring GCM uses other than extended-range weather forecasting – *e.g.*, the studies of long-term climate changes and the assessment of large-scale, deliberate or inadvertent, climate modifications. The Global Atmospheric Research Programme (GARP) also began to put more emphasis on its second objective (of understanding the physical basis of climate) in addition to its first objective (of extending the range of dynamical weather forecasting), thereby underpinning this shift.<sup>120</sup>

By the early 1970s, the first Met.O.20 5-level GCM had been prepared.<sup>121</sup> The new, faster IBM 360/195 computer became available a year later, overcoming the problem of lack of computing power and time for climate modelling. The first GCM climate sensitivity experiment, conducted by Roger Newson, was a simplified simulation of how the northern polar climate would react to a removal of the Arctic ice-cap.<sup>122</sup> The results showed that the lowered albedo of the polar cap would result in higher temperatures and disturb atmospheric circulation patterns. These were comparable to those obtained from another ice-free Arctic GCM experiment by the RAND Corporation.<sup>123</sup> RAND was then running a programme called “Climate Dynamics for Environmental Security”.<sup>124</sup> Built upon Joseph Fletcher’s previous work on polar climates, it was developed in reaction to the concern of the defence community that the US might be harmed by changes in the climate elsewhere on the globe. The Met.O.20’s simulation was also inspired by the

119 **G.D. Robinson** ‘Some Current Projects for Global Meteorological Observation’, *Q. J. Roy. Meteor. Soc.* **93** (1967): 409-18.

120 **Schneider** *Interview by the author*.

121 **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1970); **G.A. Corby et al.** ‘A General Circulation Model of the Atmosphere Suitable for Long Period Integrations’, *Q. J. Roy. Meteor. Soc.* **98** (1972): 809-32.

122 **R.L. Newson** *An Ice-Free Experiment with the Met.O.20 General Circulation Model*, Met.O.20 Technical Note, No. II/7 (Bracknell: British Met Office, 1972); ‘Response of a General Circulation Model of the Atmosphere to Removal of the Arctic Ice-cap’, *Nature* **241** (1973): 39-40.

123 **M. Warshaw and R.R. Rapp** *An Experiment on the Sensitivity of a Global Circulation Model - Studies in Climate Dynamics for Environmental Security*, RAND Paper R-0908 (Santa Monica: RAND Corporation, 1972); ‘An Experiment on the Sensitivity of a Global Circulation Model’, *J. Appl. Meteorol.* **12** (1973): 43-49.

124 RAND climate dynamics programme had 9 subprojects: (1) determination of changes in the mean state of an atmospheric model with different boundary conditions and different initial conditions; (2) experiments with the Mintz-Arakawa model to determine the direction of change; (3) experiments with a newly developed barotropic ocean model; (4) a study of the effect of turbidity and cloudiness on atmospheric radiation; (5) development of a small-scale convective cloud model; (6) establishment of new numerical analysis methods; (7) comparison of the present model with alternative models; (8) continuation of the study of climate as it has been recorded or deduced in the past; and (9) preparation of the ILLIAC IV computer to program cloud models in the future. See **R.R. Rapp** *Climate Modification and National Security*, RAND Paper R-4476 (Santa Monica: RAND Corporation, 1970)

debates over the Russian river diversion plan that Lamb helped stimulate.<sup>125</sup> However, the model was premature, and the aim of this experiment was to demonstrate the superiority of GCM over other approaches such as Lamb's rather than to produce complete assessments. Mason wrote a few years later that these results served "to illustrate the limitations of intuitive judgements in dealing with such highly interactive, nonlinear systems"<sup>126</sup>

The Met Office's first official scientific assessment of climate change was on the potential climatic impact of SSTs. This issue became hotly debated in the US. Syukuro Manabe's early study implied that the effects on the earth's temperature of added water vapour in the stratosphere would not be significant. Subsequent photochemical studies in the late 1960s suggested that water vapour exhaust from SSTs might catalytically destroy stratospheric ozone, and in turn increase the penetration of biologically harmful ultra-violet (UV) radiation.<sup>127</sup> In 1971, atmospheric chemists, Paul Crutzen at Stockholm and Harold Johnston at UC Berkeley, each claimed that NO<sub>x</sub> emission from SSTs could also play a catalytic role in the destruction of stratospheric ozone.<sup>128</sup> While Crutzen and Johnston were doing their calculations, the US Congress and Senate voted against further funding for the Boeing SST. In the end, the Boeing project was killed mainly on economic grounds, but the issue of human impact on climate received substantial coverage in the US media.<sup>129</sup> This series of events prompted the US Department of Transportation to establish the Climatic Impact Assessment Program (CIAP) in 1971.<sup>130</sup>

125 **R.L. Newson** *Interview by the author* (August 2, 2000)

126 **B.J. Mason** 'Recent Advances in the Numerical Prediction of Weather and Climate', *Proc. R. Soc. Lon. Ser.-A* **A363** (1978): 297-333. By "intuitive judgements", Mason of course meant the kind of arguments that Lamb and other climatologists had advanced.

127 **H. Harrison** 'Stratospheric Ozone with Added Water Vapor - Influence of High Altitude Aircraft', *Science* **170** (1970): 734-36. See also **L. Dotto and H. Schiff** *Ozone War* (New York: Doubleday, 1978)

128 **P. Crutzen** *On Some Photochemical and Meteorological Factors Determining the Distribution of Ozone in the Stratosphere - Effects of Contamination by NO<sub>x</sub> Emitted from Aircraft*, MISU Report AP, No. 6 (Stockholm: Meteorological Institute, 1971); **H. Johnston** 'Reduction of Stratospheric Ozone by Nitrogen Oxide Catalysts from Supersonic Transport Exhaust', *Science* **173** (1971): 517-22.

129 See **Dotto and Schiff** *Ozone War*; **M. Horwitch** *Clipped Wings - The American SST Conflict* (Cambridge: MIT Press, 1982); **J. Rosenbloom** 'The Politics of the American SST Programme - Origin, Opposition and Termination', *Soc. Stud. Sci.* **11** (1981): 403-23.

130 The US Department of Transportation first proposed CIAP in 1970, but the programme actually started in 1971. Although CIAP consisted of five major subprogrammes: 1) Engine Emissions; 2) Atmospheric Modelling; 3) Atmospheric Monitoring & Experiments; 4) Biological Monitoring & Experiments; and 5) Analysis, Integration & Assessment. CIAP concentrated primarily on the potential effects of SSTs on stratospheric ozone and accompanying health and ecological risks. However, more general issues of climate change, and of improvements in climate modelling, were also considered. See **CIAP** *Climatic Impact Assessment Program - Development and Accomplishments, 1971-1975* (Washington, DC: US Department of Transportation, 1975)



By the late 1960s, the Anglo-French SST project, Concorde, was well under way and the test flights of prototype aircrafts were being carried out in Britain and France. At first, the Met Office's Special Investigations Branch concentrated only on the problems associated with the forecasting requirements for successful SST flights such as turbulence, icing, landing or the distribution of cumulonimbus clouds.<sup>131</sup> Many dynamical meteorologists, both at the Met Office and elsewhere, were sceptical that SSTs could pose serious threats to stratospheric ozone. The conclusions of Crutzen and Johnston were based entirely on 1-D photochemical calculations, and virtually ignored atmospheric dynamics,<sup>132</sup> which meteorologists believed would dominate the stratospheric distribution of ozone and NO<sub>x</sub>.<sup>133</sup> Furthermore, despite the growing public concern over pollution, the environmental aspect of SSTs did not emerge as an important issue in Britain, except for that of sonic boom.<sup>134</sup> But Concorde was designed for international flights, and the Met Office was in due course asked to give advice on the problem of the NO<sub>x</sub>/ozone connection.<sup>135</sup> In 1972, the Met Office set up the Committee on Meteorological Effects of Stratospheric Aircraft (COMESA).<sup>136</sup>

COMESA directed a three-year multi-disciplinary project. This was Britain's first systematic attempt to organize research into climate-related issues. Unlike CIAP, in which the biological, social and economic aspects of SSTs were also studied, COMESA was confined to atmospheric research. Part of the work was contracted out to university groups,<sup>137</sup> but the overall coordination was left within the Met Office. Thus, a strong

131 **A.F. Crossley** 'Atmospheric Environment of Supersonic Transport Aircraft', *Q. J. Roy. Meteor. Soc.* **95** (1969): 784-88; **L. Sugden** 'Provision of Meteorological Service for Supersonic Transport Operations', *Q. J. Roy. Meteor. Soc.* **95** (1969): 789-94.

132 This does not mean, however, that Crutzen and Johnston were simply unaware of the importance of atmospheric dynamics. Paul Crutzen, for instance, obtained a Filosofie Licentiat (equivalent to a PhD) from Stockholm's Department of Meteorology. He was originally trained as a dynamical meteorologist although he chose the photochemistry of atmospheric ozone as his dissertation topic. Harold Johnston was a physical chemist and might not have had a full understanding of atmospheric dynamics. Nevertheless, he early on became acquainted with some knowledge of meteorology during World War II when he conducted fieldwork with the Chemical Warfare Service. **H. Johnston** *Bridge Not Attacked - Chemical Warfare Civilian Research During World War II* (River Edge: World Scientific, 2003)

133 The SMIC reports thus stated that "... the problem of stratospheric ozone distribution is very complex and must be studied in an atmospheric model in which proper treatment is given to dynamical processes [air motions] before any definitive results can be considered reliable. ...". **SMIC** *Inadvertent Climate Modification*.

134 The Met Office scientists believed that even the sonic boom could not give rise to serious safety problems. **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1965-70)

135 For instance, Britain had to persuade the US government agencies to grant landing rights for Concorde in New York.

136 **R.J. Murgatroyd** (ed) *Report of the Committee on Meteorological Effects of Stratospheric Aircraft* (Bracknell: Meteorological Office, 1975)

137 Laboratory investigations into stratospheric photochemistry were contracted out to a group of chemists at

emphasis was placed upon atmospheric modelling. The Met.O.20 devised a 13-level GCM for COMESA by adding the extra vertical resolution in the stratosphere to the existing 5-level model.<sup>138</sup> The problem was that computing constraints prohibited the GCM calculation of a full 3-D distribution of NO<sub>x</sub> with interactive photochemistry. COMESA's final estimates for SST-induced ozone depletion were more conservative than those of CIAP – calculating that several hundred Concorde, each flying five hours per day, would reduce the stratospheric ozone content by less than 0.5%.<sup>139</sup> These results, in which the British government was most interested, were based largely on laboratory investigations, observational studies, and 1-D photochemical and 2-D dynamical-chemical models.<sup>140</sup> Yet, the Met Office maintained the importance of GCM, and used the 13-level model to simulate the natural stratosphere and to study the likely climatic effects of stratospheric ozone depletion.<sup>141</sup>

### Establishment of Climatic Research Unit

In the meantime, Hubert Lamb became convinced that there was no future for him in the Met Office. He later recalled, “... [new director-general] Dr B.J. Mason ... did not seem to share Sutton's belief in the value of my qualifications or the kind of studies I was pursuing. ...”<sup>142</sup> As retirement was looming ahead in the late 1960s, he took this

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Oxford and Cambridge (led by R.P. Wayne) while experimental observations of ozone and other minor constituents were made by groups at Oxford's Clarendon Laboratory (led by John Houghton) and at the National Physical Laboratory (led by N.W.B. Stone). See *Ibid.* It is noteworthy that Paul Crutzen published his first work on the NO<sub>x</sub>/ozone problem while he was doing a post-doc at the Clarendon Laboratory. In 1971, he returned to Stockholm and began to work on the impact of SST exhausts. When in 1973 Crutzen was awarded another degree, Filsofie Doctor (equivalent to a DSc), for this work, John Houghton and R.P. Wayne were promoters for his dissertation. See **P.J. Crutzen** *Nobel Lecture - My Life with O<sub>3</sub>, NO<sub>x</sub> and Other YZO<sub>x</sub>s* (December 8, 1995)

138 R.L. Newson, S.R. Mattingly and J.S. Foot were involved in the development of COMESA 13-level GCM. See **Murgatroyd** (ed) *Report of COMESA*.

139 Interestingly, by 1979, the improved models were predicting that the emission of NO<sub>x</sub> by Concorde could slightly increase the stratospheric ozone content. See **B.J. Mason** 'Some Results of Climate Experiments with Numerical Models', in *Proceedings of the World Climate Conference* (Geneva: WMO, 1979) However, as more results became available from atmospheric chemistry research a few years later, the estimates came back to near the original figures. For later developments, see **S.H. Schneider and R. Londer** *The Coevolution of Climate and Life* (San Francisco: Sierra Club, 1984)

140 A 1-D photochemical model was built by A.F. Tuck at the Met Office while a 2-D dynamical-chemical model was constructed by R.S. Harwood from John Houghton's group. See **Murgatroyd** (ed) *Report of COMESA*.

141 Preliminary results suggested that the effects on surface temperature and rainfall were likely to be small even when large changes of composition were introduced in the stratosphere. See Newson's presentation in **A.J. Broderick and T.M. Hard** (eds) *Proceedings of the Third Conference on the Climatic Impact Assessment Program* (Washington, DC: US Department of Transportation, 1974). See also **Murgatroyd** (ed) *Report of COMESA*.

142 **Lamb** *Through All the Changing Scenes of Life*. Mason also agreed with this point. **Mason** *Interview by the*

opportunity to take a new direction in his career and started applying for university posts.<sup>143</sup> When approached by the University of East Anglia in 1971, he decided to leave the Met Office to take up the professorship. In early 1972, Lamb founded the Climatic Research Unit (CRU) within East Anglia's new School of Environmental Sciences. It was a unique experiment. Before Lamb's Unit came into being, there was only one other equivalent research institute in Western countries – the Center for Climate Research at the University of Wisconsin-Madison, which was launched by Reid Bryson in 1962 as a research centre within the Department of Meteorology.<sup>144</sup>

The advent of CRU was timely. In the early 1970s, a series of costly climatic events were reported world-wide, even more so than in the 1960s.<sup>145</sup> The prolonged drought in the Sahel zone of Africa during 1968-73, it was argued, surpassed all recorded experience. In 1972, the drought in the Soviet Union hit grain production badly, and the Peruvian anchovy industry collapsed due to El Niño.<sup>146</sup> In the following years, poor monsoons severely reduced food production in India. These incidences had profound implications for global food production and population growth. One of the CIA's internal reports produced in 1974 stated, "... climate is now a critical factor. The politics of food will become the central issue of every government".<sup>147</sup> In April 1974, worrying about the

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author .

143 Lamb was first invited to apply for the professorship of environmental sciences at Lancaster University to succeed Gordon Manley. But he turned down the offer. See **Lamb** *Through All the Changing Scenes of Life* .

144 **R.A. Bryson** *Interview by the author* (April 27, 2001)

145 Some of the climatic events reported in the early to mid 1970s were as follows:

1968-73:	Drought in the Sahel reached its peak in 1972.
1971-72:	Coldest Winter in more than two hundred years of records in parts of eastern European Russia and Turkey: River Tigris frozen over.
1972:	Greatest heat wave (in July) in the long records for north Finland and northern Russia.
1972-73:	Collapse of the Peruvian anchovy harvest as a result of El Niño.
1973:	Monsoon failure causing severe drought in India.
1973-74:	Floods beyond all previous recorded experience stretching across the central Australian desert.
1974-75:	Mildest winter in England since 1834. Virtually no ice on the Baltic.

From **Lamb** *Climate: Present, Past and Future - Vol. 2; Climate, History and the Modern World; Weather, Climate & Human Affairs* .

146 *El Niño* refers to a large scale weakening of the trade winds and warming of the surface layers in the eastern and central equatorial Pacific Ocean. El Niño events occur irregularly at intervals of 2-7 years, although the average is about once every 3-4 years. They typically last 12-18 months, and are accompanied by the *Southern Oscillation*, an inter-annual see-saw in tropical sea level pressure between the Pacific and the Indian Oceans.

147 These reports were prepared by the CIA's Office of Research and Development for its internal planning purposes and may not have been particularly influential in US policy-making. In any case, they were later released to the public and reproduced in a popular science book published in 1977. See **CIA** *A Study of Climatological Research As It Pertains Intelligence Problems* (1974); *Potential Implications of Trends in World Population, Food Production, and Climate* (1974), reprinted in **Impact Team** *The Weather Conspiracy - The Coming of the New Ice Age* (New York: Ballantine Books, 1977)

political stability of third world countries, the US Secretary of State Henry Kissinger told the UN General Assembly:

The poorest nations, already beset by man-made disasters, have been threatened by a natural one: the possibility of climatic changes in the monsoon belt and perhaps throughout the world. The implications for global food and population policies are ominous. The United States proposes that the International Council of Scientific Unions and the World Meteorological Organization urgently investigate this problem

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...

In response, the WMO's Executive Committee established the Panel of Experts on Climate Change in 1975, in order to coordinate its various activities concerning climate-related issues.<sup>149</sup>

However, the setting up of a university-based research institute that exclusively focused on climate change was a risky venture. Climatology, especially that of Lamb's version, was still seen as having a vague disciplinary identity. There were doubts about funding research in such an under-recognized area. The NERC Working Party on Atmospheric Sciences, which was responsible for allocating funds for climate research in British universities, was dominated by dynamical meteorologists and atmospheric physicists.<sup>150</sup> Whether or not this influenced the funding decision, NERC had a policy of providing funds for individual research projects, but not for establishing a new research group. It was therefore quite difficult for Lamb to get financial support from the British meteorological community. CRU was instead founded with the support of the Nuffield Foundation and Shell International Petroleum.<sup>151</sup>

Immediately after CRU's inception, Lamb published the first volume of his masterpiece

148 Quoted from **H.H. Lamb** 'Fluctuations in Climate', *Nature* **251** (1974): 568. See also **Hart and Victor** 'Scientific Elites and the Making of US Policy'.

149 **J. Gribbin** 'Wind of Change at WMO', *Nature* **250** (1974): 176. In the early 1970s, the climate-related activities of WMO were distributed to various committees; the Commission for Atmospheric Sciences, the Commission for Special Applications of Meteorology and Climatology, the Commission for Agricultural Meteorology, and the Joint Organizing Committee for GARP. For instance, while the first commission had its Working Group on the Physics of Climatic Fluctuations, the second commission had its Working Group on Climatic Fluctuations and Man.

150 The chairman of the Working Party was Graham Sutton, former director of the Met Office, who had been sympathetic to Lamb's work. But the members included dynamical meteorologists and atmospheric physicists from the Met Office and universities, including John Sawyer, P.A. Sheppard, John Houghton, R.P. Pearce and others. **NERC** *Research in the Atmospheric Sciences - Report by the Working Party on Atmospheric Sciences* (London: NERC, 1972)

151 **A. Piper** 'Lamb's Unit to the Slaughter?' *Nature* **248** (1974): 466-67.

*Climate: Present, Past and Future (Vol. 1 Fundamentals and Climate Now)*, helping the Unit build its international reputation.<sup>152</sup> Initially, there were just three or four research staff, consisting of Lamb and his ex-colleagues at the Met Office. They resumed what Lamb had been doing at the Office; by using past instrumental records, they reconstructed synoptic weather maps of Europe and the North Atlantic for the past two centuries to trace the atmospheric circulation development.<sup>153</sup> They adopted an actuarial approach in which improved knowledge of the long record of climatic behaviour was the key to understanding future natural climate variations. Similarly, they claimed, any possible effects of human activities on global climate could hardly be established without knowing the course of the fluctuations due to natural causes. Lamb was also one of the first climatologists to suggest that variations in sea surface temperatures might be related to climate variability. Research at the Unit in the early days accordingly included the study of the connection between the Southern Oscillation and global climate.<sup>154</sup>

Unfortunately for Lamb, it became clear within the first two years that grants from the Nuffield Foundation and Shell were not enough to keep CRU afloat. Lamb had been a member of the Working Group on Climatic Fluctuations and Man of the WMO Commission for Special Applications of Meteorology and Climatology, and was appointed its chairman in 1974. Counter to expectation, Lamb's credentials as a world-renowned climatologist did not translate into support for CRU in Britain. Lamb's grant applications to British government agencies were "more or less consistently turned down".<sup>155</sup> In 1973, NERC increased its support for climate research by setting up a new Working Party on World Climatology under the chairmanship of John Sawyer, then director of research and deputy to the director-general of the Met Office.<sup>156</sup> This did not bring any improvement in the Unit's financial status. Even the University of East Anglia saw CRU as a temporary research centre that might disappear after Lamb's retirement.<sup>157</sup> As a consequence, CRU scientists had to rely heavily on "soft research money", and Lamb spent much of his time fund-raising. This experience led Lamb to believe that his

152 **H.H. Lamb** *Climate: Present, Past and Future - Vol. 1 Fundamentals and Climate Now* (London: Methuen, 1972)

153 **H.H. Lamb** 'Reconstructing the Climatic Patterns of the Historical Past', *Endeavour* **33** (1974): 40-47.

154 See note 145. For CRU's work on this topic, see **P.B. Wright** *An Index of the Southern Oscillation*, CRU Research Publication, No. 4 (Norwich: University of East Anglia, 1975)

155 See **Lamb** *Through All the Changing Scenes of Life*.

156 **NERC** *Annual Report of the Natural Environment Research Council* (London: NERC, 1974)



Unit suffered greatly because of the negative attitude of the meteorological establishment, more specifically the Met Office senior scientists, towards historical climatology.<sup>158</sup>

Eventually in 1975, following an article in *Nature* on Lamb's situation, the financial help needed came from the Wolfson Foundation in Britain and later the Rockefeller Foundation in the US.<sup>159</sup> Having finally secured funding, CRU began to expand its research. In collaboration with the Met Office's Synoptic Climatology Branch (Met.O.13), the Unit produced homogenized data series of British rainfall back to the 17<sup>th</sup> century, and analysed them statistically to evaluate the causative synoptic and circulation processes.<sup>160</sup> But the major research project was to extend the synoptic mapping of Europe and the North Atlantic, going back in time beyond the period of instrumental records.<sup>161</sup> With the Rockefeller Foundation grant, Lamb employed Tom Wigley to take charge of the research.<sup>162</sup> The project required the extensive use and analysis of many different types of historical documentary records. Collaborative work with historians to inspect these records inevitably drew CRU scientists to the interaction between climate and human history. Lamb was in particular interested in dramatic climatic events such as the Little Ice Age, which he believed would provide information of importance for the present day.<sup>163</sup> In pursuing this project, Lamb wanted to develop mapping techniques that would incorporate not only the collected weather characteristics but also the resulting climatic impacts on human society, such as harvests, frosts, floods, droughts or snows.<sup>164</sup> This did

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157 **T. Davies** 'Twenty-five years of the Climatic Research Unit', *Climate Monitor* **25** (1996): 201-11.

158 In fact, the NERC report stated in 1976, "... It is highly desirable that such an independent centre [CRU] for the coordination of studies of the history of climate should be maintained ... that in the award of research grants, etc, by NERC the need for continuity of effort in such a centre should be recognised". See **NERC Reports of the Working Parties on Applied Climatology and on World Climatology** (London: NERC, 1976): 38. However, Lamb continued to believe that he was ill-treated by mainstream meteorologists. **T.M.L. Wigley Interview by the author** (August 14, 2000)

159 **Piper** 'Lamb's Unit to the Slaughter?'; **Lamb** *Through All the Changing Scenes of Life*. The grants from the Nuffield Foundation and Shell amounted to less than £60,000 over five years. However, the Wolfson Foundation provided £100,000 covering the years up to 1979, and the Rockefeller Foundation £120,000 until 1977. The Nuffield Foundation also decided to renew its grant. Interestingly, Lamb suggested that the *Nature* article was passed around US government offices in Washington, DC, and prompted the involvement of the Rockefeller Foundation.

160 **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1972-75). The tension between CRU and the Met Office continued, but at a more senior level (mostly on the part of Lamb).

161 Lamb originally hoped to extend the analyses as far back as 1,000 years ago, but this soon turned out to be too ambitious. See **Lamb** *Through All the Changing Scenes of Life*.

162 When Lamb retired in 1978, Tom Wigley became the new director. See next chapter.

163 **Lamb** *Climate: Present, Past and Future - Vol. 2*. It was believed that there was a marked return to colder conditions between the 15<sup>th</sup> and 19<sup>th</sup> centuries. This epoch of renewed glacial advance was often called the *Little Ice Age*.

164 **Lamb** *Through All the Changing Scenes of Life*; **P.D. Jones Interview by the author** (August 15, 2000) Lamb

not materialize in the end, but shows his interest in the human side of climate change.

This sense of the social relevance of climate research had been at the heart of the Unit's activities from the beginning. Lamb had always stressed the value of climate forecasting, broadly conceived, at the Met Office. He brought this with him to CRU. Unusually for a British academic institution in the early 1970s, one of CRU's four original aims was:

To investigate the possibilities of making advisory statements about future trends of weather and climate from a season to many years ahead, based on acceptable scientific methods and in a form likely to be useful for long-term planning purposes.<sup>165</sup>

CRU did not attempt to develop detailed methods for operational climate forecasting, but the idea of climate forecasting, with the maxim of “the past is the key to the future”, had a strong influence on its research.

### Coming of a New Ice Age?

In the US, some groups of scientists quickly responded to heightened public awareness of climate change. In January 1972, a number of geologists and climatologists gathered at a working conference “The Present Interglacial, How and When Will it End?” held in Brown University.<sup>166</sup> After reviewing their geological and historical data and the prospect of long-term global climatic forecasting, the conference participants concluded that the world-wide cooling, which was thought to reverse the warming trend of the 1940s, could well be an indication of the near end of an interglacial period.<sup>167</sup> George Kukla and Robert Mathews, geologists who organized the Brown conference, explained:

[T]he majority of the participants agreed to the following points: ... Global cooling and related rapid changes of environment, substantially exceeding the fluctuations

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recorded all the necessary information, including climatic impacts on society, as verbal descriptions on his maps. It is interesting to compare this with maps from GCM numerical experiments. In GCM, society, or human agencies, could only be incorporated in terms of numerical values representing their effects such as the estimation of CO<sub>2</sub> emission.

<sup>165</sup> The other three aims were; 1) To establish firmer knowledge of the history of climate in the recent and distant past; 2) To monitor and report on current climatic developments on a global scale; and 3) To identify the processes (natural and man-made/anthropogenic) at work in climatic fluctuations and the characteristic time-scales of their evolution. See **Climatic Research Unit**, *Monthly Bulletin* 1 (1972)

<sup>166</sup> **G.J. Kukla et al.** 'The End of the Present Interglacial', *Quaternary Res.* 2 (1972): 261-9. There were some participants from Europe. Kukla himself had recently moved from Czechoslovakia to Lamont-Doherty Geological Laboratory at Columbia University. However, most of the participants were from the US.

<sup>167</sup> **G.J. Kukla and R.K. Mathews** 'When Will the Present Interglacial End?' *Science* 178 (1972): 190-1.

experienced by man in historical times, must be expected within the next few millennia or even centuries. In man's quest to utilize global resources, and to produce an adequate supply of food, global climatic change constitutes a first order environmental hazard which must be thoroughly understood well in advance of the first global indications of deteriorating climate. ...<sup>168</sup>

Later in that year, Kukla and Matthews wrote a letter to President Nixon, urgently warning of global climatic disasters and calling for the organization of an international research programme into climate change.<sup>169</sup>

There were other scientists who were concerned about global cooling but who thought that human impact on climate should not be neglected. Bryson at Wisconsin-Madison kept on asserting that dust and other particles produced from industrial and agricultural activities were increasing the earth's albedo and thereby modifying climate. Like Hubert Lamb, he regarded volcanic activity as one of the prime causes of past climate changes, but suggested that natural factors alone could not account for the cooling trend since the 1940s.<sup>170</sup> It was the "human volcano" that was cooling the present climate. He warned in 1974:

[T]here is very important climate change [*i.e.* cooling] going on right now, and if the trend continues, will affect the whole human occupation of the earth – like a half billion people starving. The effects are already showing up in rather drastic ways.<sup>171</sup>

For example, Bryson claimed that monsoon failures caused by aerosol cooling resulted in the Sahelian drought disaster.<sup>172</sup> Bryson's position was controversial and not accepted by many meteorologists and climatologists. Even Stephen Schneider, who had emphasized the importance of studying the effects of increasing aerosol on global climate, said that he

168 Ibid. British geologists were generally less involved in the discussion of climate change. One notable exception was Nicholas Shackleton at Cambridge's subdepartment of Quaternary Research, who also participated in the Brown conference. In any case, it is not clear how many participants, especially non-geologists, actually endorsed the statement of Kukla and Matthews. The difference between "few millennia" and "few centuries" may not have been as significant to climatologists as it was to geologists.

169 G.J. Kukla and R.K. Matthews 'The Letter to President Nixon' (December 3, 1972)

170 See, e.g., R.A. Bryson 'A Perspective on Climatic Change', *Science* **184** (1974): 753-60. Reid Bryson received his BA in geology at Denison Univ. in 1941, and his PhD in meteorology at the University of Chicago in 1948. He first joined the faculty of the UW-Madison in 1946 at the end of his military service as a major in the Air Weather Service of the US Army Air Corps. His first appointment was in the Dept. of Geography and Geology, but then he became the founding chairman of the Dept of Meteorology. **Bryson** *Interview by the author*.

171 T. Alexander 'Ominous Changes in the World's Weather', *Fortune* **89** (1974): 90.

172 R.A. Bryson 'Drought in Sahelia - Who or What is to Blame?' *Ecologist* **3** (1973): 366-71.

shared Bryson's sense of urgency but not of certainty about the possibility of climatic disasters.<sup>173</sup> Yet, Bryson's work attracted considerable attention and raised concerns over the prospect of man-made global cooling.

While Bryson was advancing his human volcano hypothesis, in Britain Lamb was sceptical of the ability of humankind to modify such a large-scale environment as climate. Volcanic activities had been somewhat less frequent in the first half of the 20<sup>th</sup> century. Beginning in 1947, a series of eruptions occurred, which happened to coincide with the onset of the perceived cooling trend. It was difficult to evaluate their long-term influence, but Lamb believed that natural processes were more important in controlling climate. Moreover, he examined data on the frequency of westerly winds back to the 14<sup>th</sup> century and identified certain quasi-cyclic recurrences in the intensity of atmospheric circulation. By applying his actuarial approach to these findings, Lamb speculated that the decline in circulation since about the 1950s, and associated cooling, would be likely to go on to the end of this century and beyond, approaching something close to the Little Ice Age conditions.<sup>174</sup> Greater climate variability of the early 1970s (*i.e.*, more droughts, floods, cold spells and other extreme weather events), he cautioned, could be the result of this trend.<sup>175</sup>

The global cooling hypothesis might have been well-known, or even popular, among climatologists during the early to mid 1970s.<sup>176</sup> Despite this, only a very few climatologists actually published scientific articles or academic books that explicitly advocated the global cooling hypothesis. The ongoing cooling trend was itself seldom disputed, but the question of whether this meant an irrevocable long-term global change, not only for a geological but also for a historical time-scale, was rarely tackled. It was rather the media that popularized the idea of global cooling.<sup>177</sup> Particularly influential was

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173 When Schneider expressed his reservations on the matter, Bryson replied, "Have you ever had to make a forecast that someone's life depended on?" This suggests that the way Bryson conducted his research and communicated with his results to the public evidently bore the clear imprint of his forecasting experience. See **S.H. Schneider and L.E. Mesirow** *The Genesis Strategy - Climate and Global Survival* (New York: Plenum Press, 1976). See also **J. Gribbin** 'Weather Warning - You Are Now Experiencing a Climatic Change', *Nature* **252** (1974): 182-83.

174 **Lamb** *Climate: Present, Past and Future - Vol. 1*.

175 **H.H. Lamb** *The Current Trend of World Climate. A Report on the Early 1970s and a Perspective*, CRU Research Publication, No. 3 (Norwich: University of East Anglia, 1974)

176 **Ponte** *The Cooling*.

177 To give a few examples, **K. Frazier** 'Earth's Cooling Climate', *Science News* (November 15, 1969); **Anon.** 'Another Ice Age?' *Time* (June 24, 1974); **P. Gwynne** 'The Cooling World', *Newsweek* (April 28, 1975): 64.

a 2-hour TV documentary *The Weather Machine* broadcast by the BBC in 1974.<sup>178</sup> Nigel Calder, former editor of *New Scientist*, wrote its script, in which he assembled the findings of various scientists, including Kukla, Lamb and Bryson, to support the possibility of a drastic shift to the ice age conditions.<sup>179</sup> *The Weather Machine* received wide publicity, even among some meteorologists.<sup>180</sup>

Mainstream meteorologists were generally unconvinced by the alleged catastrophic global cooling, although they too took up the issue of climate change. In early 1972, the US Committee for GARP (USC-GARP), established in 1968 by NAS, appointed the Panel on Climatic Variation to lay out US research plans for the second objective of GARP.<sup>181</sup> Perhaps stimulated by the letter of Kukla and Matthews, the Panel was subsequently enlarged to survey the current state-of-the-art knowledge on climate change and to prepare an advisory document for the government. After a series of meetings, the Panel completed its review by mid 1974, and submitted a report entitled *Understanding Climatic Change: A Program for Action* to USC-GARP in early 1975.<sup>182</sup> The report recommended the establishment of international as well as national climate research programmes. However, the general tone of its report was not alarmist, neither supporting nor opposing the global cooling hypothesis, but stressing that there were large uncertainties remaining to be resolved. For instance, the report's preface stated:

In making its recommendations, the Panel is aware of what has been called the problem of “(don’t know),” *i.e.*, those who are called on to implement the program may not know that we don’t know the answers to the central questions. The presentation of this report at least makes it clear that we don’t know, and thereby reduces the exponent to unity. The successful execution of the program should remove

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178 *The Weather Machine* was also aired in the US in the winter of 1975.

179 Nigel Calder, originally trained as a physicist, himself published a short scientific paper on the ice age while preparing the TV programme. See **N. Calder** 'The Arithmetic of the Ice Ages', *Nature* **252** (1974): 216-18. He also published a book based on his script. In that book, he wrote, "... One might therefore argue that there is a virtual certainty of the next ice age starting some time in the next two thousand years. Then the odds are only about 20 to 1 against it beginning in the next 100 years. If we treat a sudden 1000-year cooling as an extra risk, over and above the ice age proper, the odds shorten to something like 10 to 1 against." **N. Calder** *The Weather Machine* (London: BBC, 1974). Later, Calder became a global warming sceptic and wrote a book advocating the theory of solar cosmic rays-climate connection. See **N. Calder** *The Manic Sun - Weather Theories Confounded* (London: Pilkington Press, 1997)

180 **D.E. Parker** *Interview by the author* (October 5, 1999)

181 **USC-GARP** *Understanding Climatic Change - A Program for Action* (Washington, DC: National Academy of Sciences, 1975)

182 *Ibid.*



at least part of the remaining “don’t know.”<sup>183</sup>

Being organized through the official NAS procedures, the Panel on Climatic Variation carried more authoritative weight than SMIC. The Panel was also more inclusive. Its review processes involved a number of geologists and observational climatologists, who had not been well represented at SMIC. As a result, the final report had a fairly long appendix on past climates.<sup>184</sup> Nevertheless, the modelling community played a dominant role in the process. The Panel was co-chaired by Yale Mintz and W. Lawrence Gates, who had led their own GCM research groups at UCLA and RAND respectively.<sup>185</sup> The Panel members included Jule Charney and Edward N. Lorenz from MIT, Syukuro Manabe and Kirk Bryan from GFDL, and Warren M. Washington from NCAR, who had all been closely involved in US GCM developments.<sup>186</sup> During the Panel’s review meetings and its preparation for the report, a key concern was how best to study climate change, and what role GCM would play in this, not the question of whether the earth’s climate was cooling or warming.<sup>187</sup>

### Slow Expansion of Climate Research at the Met Office

The USC-GARP report marked a new step in the development of GCM-based climate science. It led to a clearer recognition that climate was not controlled by the atmosphere alone but was a system comprising five main components – the atmosphere, oceans, cryosphere, land surface and biomass.<sup>188</sup> Naturally, the necessity of coupled atmospheric-oceanic GCMs in climate research began to be more strongly emphasized.<sup>189</sup>

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183 See preface in *Ibid.*

184 **USC-GARP** 'Appendix A - Survey of Past Climates', in *Understanding Climatic Change - A Program for Action* (Washington, DC: National Academy of Sciences, 1975): 127-95.

185 After receiving a PhD in meteorology from MIT, W.L. Gates was a research meteorologist at the Air Force Cambridge Research Center and then was on the faculty of the Dept of Atmospheric Sciences at UCLA, where he collaborated with Yale Mintz. He then moved to RAND to direct its climate dynamics programme. In 1975, RAND climate dynamics programme was transferred to the National Science Foundation (NSF), and Gates and his group moved to Oregon State University. **W.L. Gates** *Interview by the author* (March 27, 2001)

186 The Panel members also included W.S. Broecker (Brown, geology), G.H. Denton (Univ. of Maine, geology), and H.C. Fritts (Univ. of Arizona, dendrochronology). But their role was largely confined to the preparation of an appendix on past climates. See **USC-GARP** *Understanding Climatic Change*.

187 **Gates** *Interview by the author*.

188 The report’s schematic illustration of climate system has since been frequently reproduced in other scientific publications, along with a new concept of ‘climate system’.

189 An inter-comparison study of GCMs from different research groups was also carried out for the first time. The report contained an appendix on a comparative review of the ability of current atmospheric and oceanic GCMs

The central role of GCMs in climate research was further highlighted when WMO and ICSU convened the GARP study conference on “Physical Basis of Climate and Climate Modelling” in Stockholm in July 1974.<sup>190</sup> This conference was designed to provide a basis for the implementation of GARP’s second objective. Its important aspect was that it aimed at developing international consensus on how to model climate and its change. Faced by rising concern over potential climate threats, the political significance of climate modelling was being recognized by the participants. There was what one science reporter described as a “new sense of urgency” to construct models capable of predicting climate change.<sup>191</sup> Many participants thus showed great interest in the GCM simulations of climate change presented at the conference.<sup>192</sup> Policy discussions were largely sidelined, however, except for the call for setting up an international climate research programme. A few scientists such as Schneider attempted to raise climate policy issues more directly, without much success.<sup>193</sup>

Interestingly, there were more discussions on the topic of anthropogenic climate change at the conference. For example, Syukuro Manabe reported his results for the GCM study of CO<sub>2</sub>-doubling, while Jule Charney reviewed his GCM results for the effects of overgrazing on desert and drought in the Sahel.<sup>194</sup> Bert Bolin of Stockholm University presented a paper on the role of the carbon cycle in climate change.<sup>195</sup> This was partly because so many climate disasters within a very short period gave rise to speculation that these developments might have causes other than natural variability. Another important

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to simulate selected climate variations. **USC-GARP** 'Appendix B - Survey of the Climate Simulation Capability of Global Circulation Models', in *Understanding Climatic Change - A Program for Action* (Washington, DC: National Academy of Sciences, 1975): 196-239. A more comprehensive inter-comparison study was launched only in 1990, when the global warming issue made it important to secure the credibility of GCM results. W.L. Gates suggested that in the 1970s some modellers did not like the idea of inter-comparison. **Gates** *Interview by the author*. See also **W.L. Gates** 'AMIP - The Atmospheric Model Intercomparison Project', *B. Am. Meteorol. Soc.* **73** (1992): 1962-70.

190 The draft of the USC-GARP report served as a source document for the conference. See foreword in **GARP** *The Physical Basis of Climate and Climate Modelling - Report of the International Study Conference*, GARP Publication Series, No. 16 (Geneva: WMO, 1975)

191 **A.L. Hammond** 'Modeling the Climate - A New Sense of Urgency', *Science* **185** (1974): 1145-47.

192 However, the importance of simple models was also discussed at the conference. In fact, the concept of the hierarchy of climate models was developed and proposed to recognize the role of simple models. See, e.g., **J. Adem** 'A Critical Appraisal of Simple Climatic Models'; **S.H. Schneider** and **R.E. Dickinson** 'Climate Modelling Methodology', in *The Physical Basis of Climate and Climate Modelling* (Geneva: WMO, 1975): 142-47, 163-70. See also Chapter 4 and 6.

193 **Schneider** *Interview by the author*.

194 See **Manabe** 'The Use of Comprehensive General Circulation Modelling'; **J. Charney** 'Dynamics of Deserts and Drought in the Sahel', in *The Physical Basis of Climate and Climate Modelling* (Geneva: WMO, 1975): 171-75.

195 Bolin's research was based on simple box models, not GCMs. **B. Bolin** 'A Critical Appraisal of Models for

context was that the effects of external forcings, especially those agents under human control, were seemingly less problematic for modelling than were the effects of internal forcings or unknown periodicities in nature.

The Met.O.20 scientists took a more conservative stance. They adopted a cautious view on GCM; that is, without the models' "physical realism", any perturbation experiments (*i.e.*, climate change simulations) could not be trusted. Consequently, they focused on ensuring that their GCMs realistically simulated the presently observed climate and its short-term variability.<sup>196</sup> The 5-level GCM was extensively used to study the effects of sea surface temperature anomalies, which were believed to influence weather systems on a large scale.<sup>197</sup> Substantial efforts were made to develop and test an 11-level GCM for the GARP Atlantic Tropical Experiment (GATE), which took place in June-September 1974 with the aim of understanding the tropical atmosphere and its role in the general circulation.<sup>198</sup> During GATE, a series of extended weather predictions were performed using the Met.O.20 11-level model, covering parts of the Atlantic, Africa and South America. A group of scientists was also assigned to manage the GATE Synoptic Subprogramme Data Centre.<sup>199</sup> By the time GATE was launched, the Met.O.20 had been enlarged to accommodate around 20 scientific and 13 ancillary staff. Apart from those involved in the COMESA project, only two scientists and two programmers were working full-time on the GCM studies of climate sensitivity.<sup>200</sup> In fact, no one from the Met.O.20 attended the GARP climate modelling conference.<sup>201</sup> The only participants from the Met Office were John Sawyer, who was on the GARP Joint Organizing Committee, and Raymond Hide, head of the Office's Geophysical Fluid Dynamics Laboratory (Met.O.21).<sup>202</sup>

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the Carbon Cycle', in *The Physical Basis of Climate and Climate Modelling* (Geneva: WMO, 1975): 225-35.

196 **Mason** *Interview by the author*.

197 **P.R. Rowntree** 'The Influence of Tropical East Pacific Ocean Temperatures on the Atmosphere', *Q. J. Roy. Meteor. Soc.* **98** (1972): 290-321; *Interview by the author* (October 4, 1999)

198 **British Met Office Annual Report, 1972-75**; **B.J. Mason** 'GARP Atlantic Tropical Experiment - Review', *Nature* **255** (1975): 17-20.

199 **D.E. Parker** 'The Synoptic-scale Subprogram', *B. Am. Meteorol. Soc.* **55** (1974): 720-30.

200 **Rowntree** *Interview by the author*.

201 However, there were participants from British universities. For instance, J.S.A. Green (Imperial) and C.D. Rodgers (Oxford) were present at the climate models and the radiation and cloud processes sections respectively. See list of participants in **GARP The Physical Basis of Climate and Climate Modelling: Report of the International Study Conference**, GARP Publication Series, No. 16 (Geneva: WMO, 1975).

202 The Met.O.21 was a theoretically-oriented branch located within the Physical Research Division. Its main work was to explore fundamental dynamical processes in rotating fluids. There was not much interaction

At the Met.O.13, as Hubert Lamb left the Met Office in 1972, the Office programme was re-structured with particular emphasis on data collection and processing, rather than interpretation. The Met.O.13 carried on experimental monthly and seasonal weather forecasts using analogous methods. For this, long time-series of temperature and rainfall records over the last 100 years, and much of the northern hemisphere, were being constructed.<sup>203</sup> The data were processed with a statistical analysis, in order to reveal periodicities in their amplitude and any variations from epoch to epoch, and were assembled on magnetic tape for computer use. In 1973, the Met.O.13 embarked on a major programme for extracting sea surface temperatures from a variety of sources and subjecting them to quality control.<sup>204</sup> This was thought to complement the Met.O.20's GCM studies, leading to an improved understanding of the relation between sea surface temperature anomalies and the ensuing atmospheric anomalies.

The need for assessing climate change nonetheless continued to grow. The priority of the Met Office's climate research remained on the model development, but gradually its research focus shifted towards climate change studies. For example, when in 1973 the first oil crisis prompted the British and other Western governments to consider large-scale utilization of nuclear energy, there emerged some questions about the effects of releasing waste heat into the atmosphere and oceans. In 1975, Andrew Gilchrist, who was now head of the Met.O.20, began to investigate this issue using the 5-level GCM, in collaboration with the International Institute for Applied Systems Analysis (IIASA), Austria.<sup>205</sup> A year earlier, M.J. Molina and F.S. Rowland at the UC Irvine proposed that chlorofluorocarbons (CFCs) might also lead to ozone destruction.<sup>206</sup> As a response, using the 13-level COMESA GCM, the Met.O.20 conducted research into the climatic effects of CFCs and other stratospheric trace constituents such as aerosol, in conjunction with a national research programme coordinated by the Central Unit on Environmental Pollution, the

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between the Met.O.20 and Met.O.21.

203 **British Met Office** *Annual Report, 1972-75*. As noted above, part of this work was carried out in collaboration with CRU.

204 **C.K. Folland** *Interview by the author* (October 5, 1999)

205 **A. Gilchrist** *Some Results from the UK Meteorological Office Model Relevant to the Waste Heat Experiments*, Met.O.20 Technical Note, No. II/59 (Bracknell: British Met Office, 1975); **A.H. Murphy et al.** *The Impact of Waste Heat Release on Simulated Global Climate*, RM-76-079 (Laxenburg: IIASA, 1976). IIASA, an international research institute based in Austria, was founded in 1972 as a center for joint East-West study of problems common to modern societies.

206 **M.J. Molina and F.S. Rowland** 'Stratospheric Sink for Chlorofluoromethanes - Chlorine-Atom Catalyzed Destruction of Ozone', *Nature* **249** (1974): 810-12.

Department of the Environment.<sup>207</sup>

Soon climate change studies were in full swing around the world. In the US, the White House Domestic Council proposed in December 1974 a “United States Climate Program” with funding of \$240 million for 1975-78.<sup>208</sup> When in August 1975 the symposium on long-term climatic fluctuations was held in East Anglia, sponsored by the WMO and the International Association of Meteorology and Atmospheric Physics (IAMAP), 250 scientists from over 30 countries attended.<sup>209</sup>

In the Met Office, director-general John Mason became personally interested in the studies of climate change. In early 1976, he gave a Symons Memorial Lecture “Towards the Understanding and Prediction of Climatic Variations” at the Royal Meteorological Society, reviewing past climates, possible causes of climate change and various GCM results from the Met.O.20. He underlined the potential importance of, and the need to implement, a sustained research programme into climate change. And yet, he was very reserved in his assessment of the status of climate change research:

I think that the likelihood of major and potentially catastrophic changes in climate has been grossly exaggerated. ... Unfortunately our understanding of the mechanisms and causes of climatic trends and fluctuations is inadequate to allow of their prediction. It is not even clear whether they are brought about by internal changes in the atmospheric-oceanic system or by changes in external factors.<sup>210</sup>

On the question of global cooling, Mason acknowledged, there was a rather high probability that a long-term cooling trend might set in. However,

[T]here is no physical basis for predicting either the timing or magnitude of such changes because we do not yet understand the underlying causes. Likewise there is no real basis for the alarmist predictions of an imminent ice age which have largely been based on extrapolation of the 30-yr trend of falling temperatures in the northern

207 **British Met Office** *Annual Report, 1972-75*.

208 This eventually led to the enactment of the National Climate Program Act (NCPA) in 1978. See, e.g., **J.R. Justus** 'Mobilizing for a National Climatic Research Program - Needs and Deficiencies', in *A Primer on Climatic Variation and Change*, US Congress House Committee on Science and Technology (ed) (Washington, DC: US GPO, 1976): 148-65; **A.D. Hecht and D. Tirpak** 'Framework Agreement on Climate Change - A Scientific and Policy History', *Climatic Change* **29** (1995): 371-402.

209 **WMO/IAMAP** *Proceedings of the WMO/IAMAP Symposium on Long-term Climatic Fluctuations*, WMO series, No. 421 (Geneva: WMO, 1975)

210 **Mason** 'Towards the Understanding and Prediction of Climatic Variations', at 473, 475.



hemisphere between 1940 and 1965.<sup>211</sup>

On the other hand, although Mason repeatedly pointed out that GCMs still had serious deficiencies, he tended to have more faith in the model results. Hence, the preliminary results from the Met.O.20 13-level model were enough for him to discredit Hubert Lamb's thesis on the connection between climate and volcanic eruptions.<sup>212</sup> As part of preparing his discussion on long-term climate change, he also asked the Met.O.20 scientists to carry out GCM experiments on the effects of changes in the earth's orbit.<sup>213</sup>

Mason's position was that not only was further scientific research required, but also that this research should be carried out using complex physico-mathematical models. Until then, one should be very careful in making any claims about climate change. It was coming from this background that a few years earlier had made him decline to join *The Weather Machine* programme as its scientific advisor. Mason decided to withdraw when he found out that Nigel Calder had already written the script before consulting the Met Office scientists.<sup>214</sup> While the script did report the state-of-the-art development in GCMs, it devoted considerable space to empirical studies of climate and the possibility of a new ice age. This was unacceptable to Mason. Calder later recalled the tension between the Met Office and *The Weather Machine*'s production team:

The UK Met Office was extremely cross about our TV show. It demanded equal airtime to say that talk about a coming ice age was nonsense. For 18 months angry letters went to and fro between the BBC and the Met Office.<sup>215</sup>

Mason also believed that human impact was still too small to cause noticeable disruptions to climate. For him, the climate system was "so robust, and contains so much inherent stability through the presence of negative feedback mechanisms".<sup>216</sup>

With Mason's strong support, from 1976 onwards, the Met Office's climate change

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211 Ibid. , at 483.

212 Ibid.

213 **Mason** Interview by the author; **J.F.B. Mitchell** *The Effect on Climate of Changing the Parameters of the Earth's Orbit - Two Summer Integrations Incorporating a Fully Interactive Surface*, Met.O.20 Technical Note, No. 11/72 (Bracknell: British Met Office, 1976)

214 **Mason** Interview by the author .

215 **N. Calder** 'The Climate Wars - Reporting from the Battlefield', *CLOUD meeting at Abingdon* (April 16, 2000)

216 Quoted from **J. Gribbin** 'Man's Influence Not Yet Felt by Climate', *Nature* **264** (1976): 608.

research efforts rapidly expanded.<sup>217</sup> By around this time, the Met.O.20 had also developed new radiation schemes with a more sophisticated treatment of clouds, which were thought to be more suitable for both controlled and perturbed climate simulations.<sup>218</sup> Yet the conservative, and at the same time GCM-oriented, approach described above would dominate future climate change research at the Met Office.

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217 The Office's annual report in 1976 had a special section on climate change. See **British Met Office** 'Special Topic - Climatic Variability and Interannual Variability', in *Annual Report of the Meteorological Office, 1976* (London: HMSO, 1976): 73-85. The Met.O.20 also began ocean modelling in that year. **J.F.B. Mitchell** *One Dimensional Modelling of the Upper Layers of the Ocean*, Met.O.20 Technical Note, No. II/61 (Bracknell: British Met Office, 1976)

218 See, e.g., **G.E. Hunt and S.R. Mattingly** *Atmospheric Radiation Studies*, Met.O.20 Technical Note, No. II/31 (Bracknell: Meteorological Office, 1974); *Infrared Radiative Transfer in Planetary Atmospheres - I: Effects of Computational and Spectroscopic Economies on Thermal Heating/Cooling Rates*, Met.O.20 Technical Note No. II/60 (Bracknell: Meteorological Office, 1976); **J. Walker** *Interactive Cloud and Radiation in the 11-Layer model - Part I. Radiation Scheme*, Met.O.20 Technical Note No. II/91 (Bracknell: Meteorological Office, 1977); **P.R. Rowntree and J. Walker** *Radiation Schemes in the Met.O.20 Models*, Met.O.20 Technical Note No. II/93 (Bracknell: Meteorological Office, 1977)

## Chapter 4. Building the Basis for the Modern Science of Global Warming

By the early 1950s, the CO<sub>2</sub>-induced global warming hypothesis, revived by the British amateur climatologist Guy S. Callendar,<sup>1</sup> was relatively well known to climatologists and meteorologists.<sup>2</sup> Callendar's basic claim was that, if the atmospheric CO<sub>2</sub> content increased, with other factors being in equilibrium and with a dry sky, the greenhouse effect would be enhanced, and this would accordingly raise the earth's mean surface temperature. While his idea was quite straightforward and might have been readily accepted, it was not believed to be significant. For the long-wave absorption of CO<sub>2</sub> was thought to be overlapped by that of water vapour, thus blocking the enhanced greenhouse effect.<sup>3</sup> Soon atmospheric physicists contended that CO<sub>2</sub> had narrow absorption lines,<sup>4</sup> which meant that the radiative effects of added CO<sub>2</sub> might not be cancelled out by water vapour. Callendar's calculations were very crude, however. It was also generally believed at the time that most of the additional CO<sub>2</sub> produced by the burning of fossil fuels would be absorbed by the oceans.<sup>5</sup> Besides, for many scientists, the very possibility of anthropogenic climate change was too remote.<sup>6</sup> Even though Callendar's work was not dismissed straight away, therefore, it generated little interest from climatologists and meteorologists.

As briefly noted in the previous chapters, a few other scientists sought to rekindle Callendar's argument. In 1953, Gilbert Plass, a physicist at John Hopkins, tentatively started to make elaborate radiation calculations on the effects of increased CO<sub>2</sub> on

1 See, for example, **G.S. Callendar** 'The Artificial Production of Carbon Dioxide and Its Influence on Temperature', *Q. J. Roy. Meteor. Soc.* **64** (1938): 223-37; 'Can Carbon Dioxide Influence Climate?' *Weather* **4** (1949): 310-14.

2 **C.E.P. Brooks** 'Geological and Historical Aspects of Climatic Change', in *Compendium of Meteorology*, T.F. Malone (ed) (Boston: American Meteorological Society, 1951): 1004-18.

3 Ibid. .

4 **S.R. Weart** 'The Carbon Dioxide Greenhouse Effect', in *The Discovery of Global Warming*, <http://www.aip.org/history/climate> (recorded copy at American Institute of Physics, 2001): p. CO2-7 of 08/01/01 text.

5 See **S.R. Weart** 'Global Warming, Cold War, and the Evolution of Research Plans', *Hist. Stud. Phys. Biol.* **27** (1997): 319-56, and references therein.

6 In reviewing various theories of climatic change, C.E.P. Brooks briefly mentioned the work of Callendar and wrote, "... But during the past 7000 years there have been greater fluctuations of temperature without the intervention of man, and there seems no reason to regard the recent rise as more than a coincidence." See **Brooks** 'Geological and Historical Aspects of Climatic Change' .

climate.<sup>7</sup> Using improved infrared spectra and an electronic computer, he produced final results in 1956, in which he computed that a doubling of CO<sub>2</sub> would warm the earth's surface by 3.6 °C for a cloudless atmosphere.<sup>8</sup> Just as important was the work by Roger Revelle and Hans Suess at the Scripps Institution of Oceanography. In 1957, they published their recent work on the CO<sub>2</sub> exchange between the atmosphere and the ocean, and the resultant increase in atmospheric CO<sub>2</sub>.<sup>9</sup> Their estimate for a net increase was smaller than Callendar had proposed. On the other hand, they pointed out that, owing to a seawater buffering mechanism, the oceanic uptake of CO<sub>2</sub> could be more limited than commonly supposed. Building upon the work of Callendar and Plass, they suggested that humanity might already be carrying out a "large-scale geophysical experiment".<sup>10</sup>

These studies led Callendar to update his previous analyses and publish a few more papers reaffirming his thesis.<sup>11</sup> Yet the enhanced greenhouse effect was not immediately seen as the major climatological question of the time. A range of problems remained, for example as to the extent to which anthropogenic CO<sub>2</sub> would be absorbed by the oceans – *i.e.*, if CO<sub>2</sub> was really accumulating in the atmosphere. A dry atmosphere assumption was unrealistic. In the real atmosphere, the radiative processes would be further obscured by feedback from water vapour and clouds.<sup>12</sup> The warming trend also needed to be demonstrated to establish that atmospheric CO<sub>2</sub> was having a real effect on climate. Several climatological studies indicated that the earth was getting warmer, but careful examinations of the global temperature trend were rare. The next few years witnessed a steady growth in research related to the CO<sub>2</sub>-induced global warming hypothesis. Nevertheless, research activities were carried out sporadically around the above three closely linked but distinct topics – carbon cycle, atmospheric radiation, and global

7 **G.N. Plass** 'The Carbon Dioxide Theory of Climatic Change', *B. Am. Meteorol. Soc.* **34** (1953): 80; 'Some Problems in Atmospheric Radiation', in *Proceedings of the Toronto Meteorological Conference* (London: Royal Meteorological Society, 1954)

8 **G.N. Plass** 'The Influence of the 15  $\mu$  Carbon Dioxide Band on the Atmospheric Infra-Red Cooling Rate', *Q. J. Roy. Meteor. Soc.* **82** (1956): 310-24; 'The Carbon Dioxide Theory of Climatic Change', *Tellus* **8** (1956): 140-54; 'Effect of Carbon Dioxide Variations on Climate', *Am. J. Phys.* **24** (1956): 376-87. This was somewhat larger than Callendar's estimate of a 2 °C warming. See **Callendar** 'The Artificial Production of Carbon Dioxide and Its Influence'.

9 **R. Revelle and H.E. Suess** 'Carbon Dioxide Exchange between Atmosphere and Ocean and the Question of an Increase of Atmospheric CO<sub>2</sub> during the Past Decades', *Tellus* **9** (1957): 18-27.

10 *Ibid.*

11 **G.S. Callendar** 'The Effect of Fuel Combustion on the Amount of Carbon Dioxide in the Atmosphere', *Ibid.* **9** (1957): 421-22; 'On the Amount of Carbon Dioxide in the Atmosphere', *Tellus* **10** (1958): 243-48; 'On the Present Climatic Fluctuation', *Meteorol. Mag.* **87** (1958): 204-07.

12 For instance, the amount of water vapour and clouds could change due to temperature changes brought

temperature trend.

## Towards Global Warming Research

While Revelle and Suess were preparing their paper on atmospheric CO<sub>2</sub> in 1956, the International Geophysical Year (IGY) was being formulated.<sup>13</sup> Harry Wexler, director of the US Weather Bureau's Division of Meteorological Research, was planning to allocate some of the Bureau's IGY funding to support CO<sub>2</sub> measurements at remote locations. With this money, Revelle and Suess hired a geochemist, Charles D. Keeling, to set up an atmospheric CO<sub>2</sub> monitoring station at Mauna Loa Observatory, Hawaii.<sup>14</sup> A similar move was made in Sweden by Carl-Gustav. Rossby, director of Stockholm University's International Meteorological Institute (IMI). Rossby had lately become interested in atmospheric chemistry. Exploiting the opportunity opened up by the IGY, he created a research group centred on a soil chemist, Erik Eriksson, and launched investigations into various air- and water-borne chemicals including CO<sub>2</sub>.<sup>15</sup> In 1956, following Rossby's recommendation, Bert Bolin began to apply his mathematical modelling techniques to the carbon cycle.<sup>16</sup>

Within a few years, Bolin and Eriksson devised a more refined seawater buffering mechanism, from which they inferred that fossil fuel combustion would undoubtedly give rise to a substantial increase of atmospheric CO<sub>2</sub> in the next decades.<sup>17</sup> This conclusion

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about by increasing CO<sub>2</sub> in the atmosphere. In return, this would affect the earth's surface temperature.

13 The IGY was one of the major international scientific cooperation programmes in the 1950s. Jointly organized by WMO ICSU, more than 10,000 scientists from 67 nations participated in the IGY. Its objective was to observe geophysical phenomena and to secure data over the globe during the 18-month period from July 1957 to December 1958, when a maximum in the solar sunspot cycle was expected. See **S. Chapman** *IGY, Year of Discovery - The Story of the International Geophysical Year* (Ann Arbor: University of Michigan Press, 1959)

14 **C.D. Keeling** 'Rewards and Penalties of Monitoring the Earth', *Annu. Rev. Energ. Env.* **23** (1998): 25-82.

15 **B. Bolin** 'Carl-Gustaf Rossby - The Stockholm period 1947-1957', *Tellus* **51** (1999): 4-12. As noted in Chapter 2, Rossby had headed the department of meteorology at MIT and Chicago before coming back to Sweden in 1947.

16 Rossby drew Bolin's attention particularly to the resident time of atmospheric CO<sub>2</sub>. See *Ibid.*; **H. Taba** 'The Bulletin Interviews - B. Bolin', *WMO Bulletin* **37** (1988): 233-44. It is noteworthy that about 30 years later Bolin became the IPCC's first chairman. He completed his PhD in dynamical meteorology in 1956 under Rossby's supervision. He then became the new director of Stockholm's Meteorological Institute in 1957 when Rossby suddenly died of a heart attack. During his PhD years, Bolin had been involved in developing the first Swedish numerical (barotropic) weather prediction model. See **H. Rodhe** 'Bert Bolin and His Scientific Career', *Tellus* **43** (1991): 3-7.

17 **B. Bolin and E. Eriksson** 'Changes in the Carbon Dioxide Content of the Atmosphere and Sea due to Fossil Fuel Combustion', in *The Atmosphere and the Sea in Motion*, B. Bolin (ed) (New York: Rockefeller Institute Press, 1959): 130-42; **B. Bolin** 'Atmospheric Chemistry and Broad Geophysical Relationships', *P. Natl. Acad. Sci. USA* **45** (1959): 1663-72.



was buttressed by Keeling's Scripps CO<sub>2</sub> monitoring, now supported by the National Science Foundation (NSF). In 1961, Charles Keeling took a leave of absence from Scripps and spent a year at the IML.<sup>18</sup> There, with Bolin's help, he assembled and analysed all of the Scripps CO<sub>2</sub> data until 1962. Although there was a strong seasonal cycle in CO<sub>2</sub> concentration, they were able to construct a year-to-year increase in CO<sub>2</sub> from the data, and deduced that roughly half of the CO<sub>2</sub> from fossil fuel was accumulating in the atmosphere.<sup>19</sup>

Things were more complicated in the radiation studies of atmospheric CO<sub>2</sub>. During the early 1960s, the work of Plass came under criticism. The MIT meteorologist Lewis Kaplan questioned the cloudless sky assumption and the adequacy of his radiation calculation methods, and asserted that Plass overestimated the CO<sub>2</sub>-induced rise of surface temperature.<sup>20</sup> In revisiting the overlap between water vapour and CO<sub>2</sub> absorption, Russian atmospheric physicists Kirill Ya Kondratyev and H.I. Niilisk likewise maintained that, if this were properly allowed for, Plass' temperature change estimate would be noticeably diminished.<sup>21</sup> In 1962, Fritz Möller in Germany tried to clarify the issue by considering both the effects of water vapour and clouds and that of their feedback.<sup>22</sup> His calculations showed that the surface temperature change was so sensitive to feedback that meaningful temperature sensitivity could not be easily derived. Möller thus concluded that "the entire theory of climatic changes by CO<sub>2</sub> variations is becoming very questionable".<sup>23</sup> But these results did not directly refute the basic argument of Callendar

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18 **Keeling** 'Rewards and Penalties'.

19 They first presented their work at a symposium held in Utrecht in 1962, but later published it as an article in 1963. **B. Bolin and C.D. Keeling** 'Large-scale Atmospheric Mixing as Deduced from Seasonal and Meridional Variations of Carbon Dioxide', *J. Geophys. Res.* **68** (1963): 3899-920. See also note 31.

20 **L.D. Kaplan** 'The Influence of Carbon Dioxide Variations on the Atmospheric Heat Balance', *Tellus* **12** (1960): 204-08. This paper caused a rather hot debate between Kaplan and Plass. In fact, Plass did calculate the temperature change under cloudy sky conditions (2.5 °C instead of 3.6 °C). Kaplan's criticism seems to have been based more upon Plass' calculation methods and the way Plass presented his work. See **G.N. Plass** 'Letters to the Editor', *Tellus* **13** (1961): 296-300; **L.D. Kaplan** 'Reply', *Tellus* **13** (1961): 301-02.

21 From a summary in **F. Möller** 'On the Influence of Changes in the CO<sub>2</sub> Concentration in the Air on the Radiation Balance of the Earth's Surface and on Climate', *J. Geophys. Res.* **68** (1963): 3877-86. It is noteworthy that, unlike Kondratyev and Niilisk, Kaplan did not criticize Plass' assumption that water vapour would not diminish the radiative effects of CO<sub>2</sub>.

22 Ibid. In 1961, Plass also considered the role of water vapour and cloud feedback, albeit qualitatively. But he believed that their signs would be positive, amplifying the enhanced greenhouse effect by CO<sub>2</sub>. See **G.N. Plass** 'Influence of Infrared Absorptive Molecules on Climate', *Ann. NY. Acad. Sci.* **95/1** (1961): 61-71.

23 With fixed cloudiness, he obtained a global warming of 1.5 °C for a constant absolute humidity, and of 9.6 °C for a constant relative humidity. See **Möller** 'On the Influence of Changes in the CO<sub>2</sub> Concentration'. This work was also first presented at the Utrecht symposium in 1962. See note 31. *Absolute humidity* is a measure of the actual amount of water vapour in the atmosphere, regardless of temperature, and is expressed as the ratio of the mass of water vapour present per unit volume of moist air. The ratio of the amount of water vapour in the

and Plass – their calculations all yielded a positive temperature change for a doubling of CO<sub>2</sub>.<sup>24</sup> If anything, they paved the way for the future development of simple climate models, which were later used extensively for the study of CO<sub>2</sub>-induced global warming.

What made scientists more hesitant to adopt the global warming hypothesis was the temperature trend. In 1961, Guy Callendar himself published the analysis of temperature records from over 400 stations world-wide.<sup>25</sup> Based on this analysis, he insisted that the warming trend since the late 19<sup>th</sup> century described by previous studies continued until 1950. Callendar acknowledged the role of solar variations and volcanic dust in the short-term fluctuations, but presumed that the long-term trend of rising temperature could be attributed to increased CO<sub>2</sub>.<sup>26</sup> However, the stations he used were sparsely spread over the world, especially in the Southern Hemisphere, and the quality of data collection in these stations was not homogeneous. The calculation of zonal and annual “global” mean temperatures was no less problematic.<sup>27</sup> Furthermore, a new analysis cast doubt on the warming trend in recent years. Around that time, J. Murray Mitchell at the Office of Climatology of the US Weather Bureau had completed similar empirical studies.<sup>28</sup> His data sets again reconstructed a world-wide warming since the late 19<sup>th</sup> century. Yet the analysis up to 1959 suggested that this trend, marked in the early 1940s, had been replaced by moderate cooling. After attempting to quantify each contribution from solar variability, volcanic dust and anthropogenic CO<sub>2</sub>, Murray Mitchell came to the conclusion that none of these, alone or combined, could account for the latest cooling.<sup>29</sup>

Despite many uncertainties, these researches became noticed by other climatologists

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atmosphere relative to the maximum amount that it can hold at the given temperature and pressure is called the *relative humidity*.

24 Ibid.

25 **G.S. Callendar** 'Temperature Fluctuations and Trends over the Earth', *Q. J. Roy. Meteor. Soc.* **87** (1961): 1-12.

26 Others argued that solar variations and volcanic dust could have long-term climatic effects. See chapters in **H. Shapley** (ed) *Climatic Change - Evidence, Causes, and Effects* (Cambridge: Harvard University Press, 1953)

27 It should be noted, though, that Tom Wigley *et al.* later regarded Callendar's temperature series as one of “the first reasonably reliable estimates of large scale area average temperatures”. See **T.M.L. Wigley et al.** 'Empirical Climate Studies- Warm World Scenarios and the Detection of Climatic Change Induced by Radiatively Active Gases', in *The Greenhouse Effect, Climatic Change and Ecosystems*, B. Bolin *et al.* (eds), SCOPE 29 (New York: John Wiley & Sons, 1986): 271-323. This point was also made by Weart. See **S.R. Weart** 'The Modern Temperature Trend', in *The Discovery of Global Warming*, <http://www.aip.org/history/climate> (recorded copy at American Institute of Physics, 2001): p. ModernTemps-4 of 08/01/01 text.

28 Both Callendar and Mitchell corrected the temperatures taken in populated areas such as cities for the “heat island effect”. See **J.M. Mitchell** 'Recent Secular Changes of Global Temperature', *Ann. NY. Acad. Sci.* **95/1** (1961): 235-50.

29 Ibid. .

and meteorologists. The international conferences played an important role in this process. In 1961, the New York Academy of Sciences and the American Meteorological Society (AMS) jointly sponsored a conference on “Solar Variations, Climatic Change, and Related Geophysical Problems”.<sup>30</sup> At the conference, the climatic effects of CO<sub>2</sub> were brought up by a number of participants including Plass and Mitchell. In the following year, the work of Möller, Eriksson, Bolin and Keeling on atmospheric CO<sub>2</sub> was presented at the international symposium on “Trace Gases and Natural and Artificial Radioactivity in the Atmosphere”, held in Utrecht under the joint auspices of the International Union of Geodesy and Geophysics (IUGG) and the World Meteorological Organization (WMO).<sup>31</sup>

At this stage, though, the scope of research was rather limited. There were only a handful of scientists and research groups carrying out investigations related to the global warming hypothesis. They knew one another’s work fairly in details – they published in the same journals, and were frequently cross-cited.<sup>32</sup> As mentioned, they often presented at the same conferences, and in some cases collaborated with one another. Even so, they dealt with specific aspects of the hypothesis, from the viewpoint of their own local technical interests. It is well documented that the seminal work of Revelle and Suess on carbon exchanges stemmed from their physical oceanographic research, <sup>14</sup>C tracer studies in particular.<sup>33</sup> Rossby’s atmospheric chemistry group at the IMI also did not take up, in the beginning, the issue of CO<sub>2</sub>-induced climate change. CO<sub>2</sub> was just one of many atmospheric trace chemicals that were studied. Rossby’s original interest in atmospheric CO<sub>2</sub> lay in using its variations to track the movement of air masses.<sup>34</sup> Bolin subsequently broadened the IMI’s CO<sub>2</sub> research, but was in no hurry to discuss its climatic effects. Similarly, Möller and Mitchell each approached the issue from quite different sub-

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30 **R.W. Fairbridge** (ed) *Solar Variations, Climatic Change, and Related Geophysical Problems*, Ann. NY. Acad. Sci. **95/1** (New York: New York Academy of Sciences, 1961)

31 The papers presented in this symposium were later published in the July 1963 issue (volume 68) of *Journal of Geophysical Research*.

32 Plass and Callendar published and discussed their CO<sub>2</sub> work in *Quarterly Journal of the Royal Meteorological Society*. Callendar and Mitchell also communicated through the same journal. Perhaps more importantly, Callendar, Plass, Kaplan, Bolin, Eriksson, Revelle and Suess all published their CO<sub>2</sub> work in *Tellus*, a journal of the Swedish Geophysical Society. This journal was initiated in 1949 and edited by Rossby. After Rossby’s death in 1957, *Tellus* continued to be edited by the IMI staff.

33 **D.M. Hart and D.G. Victor** ‘Scientific Elites and the Making of US Policy for Climate Change Research, 1957-74’, *Soc. Stud. Sci.* **23** (1993): 643-80; **S.R. Weart** ‘Roger Revelle’s Discovery’, in *The Discovery of Global Warming*, <http://www.aip.org/history/climate> (recorded copy at American Institute of Physics, 2001): p. Revelle-3~5 of 08/01/01 text.

34 **Bolin** ‘Carl-Gustaf Rossby’; **Keeling** ‘Rewards and Penalties’ . For the IMI’s work in this direction, see, e.g., **S. Fonselius et al.** ‘Carbon Dioxide Variations in the Atmosphere’, *Tellus* **8** (1956): 176-83.

disciplinary standpoints. It took time before these studies of diverse origins came to be identified collectively as one set of research activities.

## Global Warming as Human Intervention in Nature

### *Atmospheric CO<sub>2</sub> as a Pollutant*

An important impetus for the development of global warming research was concerns about the large-scale effects of atmospheric pollution. Nuclear fallout from bomb testing was one such pollutant that caused public anxiety in the 1950s and early 1960s. It was this issue that persuaded the US Office of Naval Research (ONR) and the Atomic Energy Commission (AEC) to fund <sup>14</sup>C tracer studies by Revelle and Suess.<sup>35</sup> The organization of the 1962 IUGG/WMO Utrecht symposium noted above was another example of scientific response to atmospheric pollution issues. At the IMI in Stockholm, too, early research grew out of an interest in the deposition of radioactive trace chemicals as well as other atmospheric pollutants. In summarizing atmospheric chemistry research at the IMI in 1959, Bolin wrote:

[T]he increasing activities of man are now becoming important factors also on the global scale. Man is interfering more and more with nature. We need to know more about where and how this occurs.<sup>36</sup>

This environmental awareness prompted a general debate on human impact on climate,<sup>37</sup> helping to bring not only attention but also a new meaning to the enhanced greenhouse effect. Previously, it was seen as just an interesting scientific question. Revelle and Suess stated in 1957 that CO<sub>2</sub>-induced changes “if adequately documented, may yield a far-reaching insight into the processes determining the weather and climate”.<sup>38</sup> During the early 1960s, it began to be framed as an environmental issue as well. In 1963, the Conservation Foundation, an environmental NGO, invited Plass, Revelle, Keeling and other scientists to attend a conference on this very issue. The conclusion reached at the conference was that a doubling of CO<sub>2</sub> would produce a warming of 3.8 °C and could

35 **Hart and Victor** 'Scientific Elites and the Making of US Policy'; **Weart** 'Roger Revelle's Discovery'.

36 **Bolin** 'Atmospheric Chemistry and Broad Geophysical Relationships'. See also **Bolin** 'Carl-Gustaf Rossby'.

37 For early discussions on human impact on climate, see Chapter 3.

38 **Revelle and Suess** 'Carbon Dioxide Exchange between Atmosphere and Ocean'.

result in potentially harmful environmental consequences.<sup>39</sup>

A more systematic appraisal followed in 1964. The US President's Science Advisory Committee (PSAC) set up the Environmental Pollution Panel and initiated the preparation of its report *Restoring the Quality of Our Environment*. As part of this effort, a sub-panel of scientists organized by Revelle reviewed the existing state of knowledge on atmospheric CO<sub>2</sub>.<sup>40</sup> The result was the chapter "Carbon Dioxide from Fossil Fuels – the Invisible Pollutant".<sup>41</sup> This chapter conceded that the temperature trend did not show a global warming beyond 1940. It nonetheless pointed to the persistent increase in atmospheric CO<sub>2</sub>, and claimed that this might trigger significant climate changes, which could be "deleterious from the point of view of human beings".<sup>42</sup>

The PSAC report did not have wider impacts. It was important in that it brought atmospheric modellers onto the scene. One of Revelle's sub-panel members was Joseph Smagorinsky, director of the Geophysical Fluids Dynamics Laboratory (GFDL), a leading centre of GCM development.<sup>43</sup> Not surprisingly, while admitting that quantitative prediction was currently not possible, the report repeatedly emphasized the implication of advances in physico-mathematical modelling for global warming research. At the time, progress in numerical weather prediction models was rapidly transforming the entire field of meteorological sciences into a highly computational discipline.<sup>44</sup> Previous studies were appreciated by the meteorological community. With the language of more advanced dynamical meteorology, however, the studies of CO<sub>2</sub>-induced global warming were becoming more respectable to mainstream meteorologists.

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39 **Conservation Foundation** *Implications of Rising Carbon Dioxide Content of the Atmosphere*, (Held in New York in March 12 1963). This volume was prepared as "a statement of the consensus of the conference".

40 By then, Revelle had moved to Harvard as first director of its Center for Population Studies. See **R. Dorfman and P.P. Rogers** *Science with a Human Face - In Honor of Roger Randall Revelle* (Cambridge: Harvard University Press, 1997)

41 This report, published in 1965, was the first US government document to recognize CO<sub>2</sub>-induced global warming as a potential environmental problem. See **R. Revelle et al.** 'Carbon Dioxide from Fossil Fuels - The Invisible Pollutant', in *Restoring the Quality of Our Environment* (Washington, DC: The White House, 1965): 111-33.

42 Ibid. For the work of Keeling cited in this report, see **J.C. Pales and C.D. Keeling** 'Concentration of Atmospheric Carbon Dioxide in Hawaii', *J. Geophys. Res.* **70** (1965): 6053-76; **C.W. Brown and C.D. Keeling** 'Concentration of Atmospheric Carbon Dioxide in Antarctica', *J. Geophys. Res.* **70** (1965): 6077-85.

43 Other scientists involved were Keeling and H. Craig from Scripps Institution of Oceanography, who were both carrying out research into the carbon cycle, and W. Broecker, a geologist from Columbia's Lamont Geological Observatory. It is interesting to note that, by this time, Plass had moved away from global warming research. See **Weart** 'Global Warming, Cold War, and the Evolution of Research Plans'.

44 See Chapter 2.



It is noteworthy that the tone of Revelle's chapter was still very much that of a technological optimist – the report mentioned several times the possibility of artificially modifying climate to curtail CO<sub>2</sub>-induced warming.<sup>45</sup> Interestingly, Smagorinsky was then on the US National Academy of Sciences (NAS) Panel on Weather and Climate Modification. This panel was established to assess deliberate modifications, but because of growing environmental awareness, it also touched upon inadvertent alterations of the atmosphere.<sup>46</sup> One of the key issues was the climatic effect of water vapour emissions from supersonic transports (SSTs). A GCM, which could take account of atmospheric dynamics, was not yet quite ready to be used. Syukuro Manabe at GFDL used instead a 1-D radiative-convective model (RCM) to investigate the issue.<sup>47</sup> It was for this modelling capacity to simulate the radiative properties of atmospheric trace gases that Smagorinsky was invited by Revelle's sub-panel. His involvement in the PSAC, in turn, led the NAS to request GFDL to execute RCM simulations on the effects of increasing CO<sub>2</sub> concentrations on global climate.<sup>48</sup>

Unlike Möller's computation which only modelled the radiation balance of the earth's surface, Manabe's RCM incorporated that of the atmosphere as a whole by including the mixing effects of vertical heat transport by atmospheric motions. Assuming a fixed absolute humidity and 50% cloud cover, Manabe and his co-workers calculated that a doubling of CO<sub>2</sub> would raise the earth's annual mean surface temperature by 1.2 °C, lower than earlier figures.<sup>49</sup> These preliminary results served as the basis for the CO<sub>2</sub> section of the NAS Panel report.<sup>50</sup> They were also submitted to Revelle's sub-panel.

Actually, Manabe and Smagorinsky did not yet regard the increase in atmospheric CO<sub>2</sub>

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45 **Revelle et al.** 'Carbon Dioxide from Fossil Fuels - The Invisible Pollutant'.

46 **US NRC** *Weather and Climate Modification - Problems and Prospects, Vol. 2: Research and Development* (Washington, DC: National Academy of Sciences, 1966). See also **C. Kwa** 'The Rise and Fall of Weather Modification - Changes in American Attitudes Towards Technology, Nature, and Society', in *Changing the Atmosphere - Expert Knowledge and Environmental Governance*, C.A. Miller and P.N. Edwards (eds) (Cambridge: MIT Press, 2001): 135-65.

47 **S. Manabe and R.F. Strickler** 'Thermal Equilibrium of the Atmosphere with a Convective Adjustment', *J. Atmos. Sci.* **21** (1964): 361-85; **US NRC** *Weather and Climate Modification, Vol. 2*.

48 At one of the NAS Panel meetings, knowing Smagorinsky's participation in the PSAC, E.P. Todd from NSF asked him, "What about this business of CO<sub>2</sub>? Do you really know how CO<sub>2</sub> changes and affects the atmosphere?" After the discussion, Smagorinsky came back to GFDL and asked Manabe to run RCM experiments on CO<sub>2</sub>. See **J. Smagorinsky** *Interview by S.R. Weart* (AIP Oral History Project: March 1, 1989).

49 However, they noted that the temperature estimate would increase if the distribution of relative humidity were taken into account. See **US NRC** *Weather and Climate Modification, Vol. 2*.

50 Manabe first presented these results at the NAS Panel's 12<sup>th</sup> meeting in the summer of 1965. The NAS Panel's final report was published in early 1966. See *Ibid.*

as a pressing environmental problem.<sup>51</sup> Although Manabe had been aware of the global warming hypothesis since the early 1960s, through his collaboration with Möller,<sup>52</sup> he was preoccupied with developing the radiative transfer scheme (*i.e.*, RCM) for GFDL's GCM.<sup>53</sup> As a result, his interest in CO<sub>2</sub> was limited to this context. Being involved in higher-level discussions, Smagorinsky might have had a broader view. Still, it was more important for him that CO<sub>2</sub>-induced climate change was a good exemplar, demonstrating the need for GCM developments.<sup>54</sup> When in 1967 Manabe and his colleague Richard T. Wetherald published the results of RCM simulations, this limited view was evident. They showed that the computed temperature change due to a doubling of CO<sub>2</sub> was 2.3 °C with fixed relative humidity and cloudiness, but affirmed:

[T]he primary objective of our study of radiative convective equilibrium is the incorporation of radiative transfer into the general circulation model of the atmosphere.<sup>55</sup>

In any event, concerns over atmospheric pollution were steadily mounting. With the apparently continuous rise of atmospheric CO<sub>2</sub>,<sup>56</sup> the scientific community increasingly characterized CO<sub>2</sub>-induced climate change as an environmental issue. In 1968, the AAAS organized a symposium on “Global Effects of Environmental Pollution”. CO<sub>2</sub> was one of the main pollutants on the table, and Manabe and Mitchell were invited to give

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51 Even Revelle did not think that increased CO<sub>2</sub> was posing a serious threat to humankind. He claimed in 1966, “Our attitude toward the changing content of carbon dioxide in the atmosphere that is being brought about by our own actions should probably contain more curiosity than apprehension”. Quoted from **Hart and Victor** ‘Scientific Elites and the Making of US Policy’.

52 **S. Manabe and F. Möller** ‘On the Radiative Equilibrium and Heat Balance of the Atmosphere’, *Mon. Weather Rev.* **89** (1961): 503–32. Möller was invited by Smagorinsky to help Manabe develop the radiative transfer scheme for GFDL's GCM. **S. Manabe** *Telephone interview by the author* August 28, 2000; *Interview by P.N. Edwards* (AMS Tape Recorded Interview Project: March 14, 1998). Also, as early as in 1961, Mitchell consulted Manabe on the global warming hypothesis. See **Mitchell** ‘Recent Secular Changes of Global Temperature’.

53 See Chapter 3. A mathematical treatment of the radiative effects of atmospheric CO<sub>2</sub> did, of course, play an important part in the formulation of the radiative transfer scheme.

54 Smagorinsky continued to advocate the potential applications of GCM throughout the 1960s and 1970s. See, *e.g.*, **J. Smagorinsky** ‘Numerical Simulation of the Global Atmosphere’, in *The Global Circulation of the Atmosphere*, G.A. Corby (ed) (London: Royal Meteorological Society, 1969): 24–41; ‘Global Atmospheric Modeling and the Numerical Simulation of Climate’, in *Weather and Climate Modification*, W.N. Hess (ed) (New York: John Wiley & Sons, 1974): 633–86.

55 **S. Manabe and R.T. Wetherald** ‘Thermal Equilibrium of Atmosphere with a Given Distribution of Relative Humidity’, *J. Atmos. Sci.* **24** (1967): 241–59. Eriksson also pointed out that the contribution of Manabe and his colleagues to the study of atmospheric CO<sub>2</sub> was “a mere by-product of their thorough numerical investigation of the thermal balance of the atmosphere.” **E. Eriksson** ‘Air-Ocean-Icecap Interactions In Relation To Climatic Fluctuations And Glaciation Cycles’, in *Causes of Climatic Change - A Collection of Papers Derived from the INQUA-NCAR Symposium* (Boston: American Meteorological Society, 1968): 68–92.

56 **Pales and Keeling** ‘Concentration of Atmospheric Carbon Dioxide in Hawaii’; **Brown and Keeling** ‘Concentration of Atmospheric Carbon Dioxide in Antarctica’.

presentations on its climatic effects.<sup>57</sup> An organiser of the symposium, S. Fred Singer, who later became a vocal critic of the global warming hypothesis, even portrayed CO<sub>2</sub> as a “classic example” of pollutants that had world-wide effects.<sup>58</sup> Outside the US, Bolin, who carried on his carbon cycle research, was active in framing the atmospheric CO<sub>2</sub> increase as a “global air pollution problem”.<sup>59</sup> The WMO also took a deep interest in the subject, and gradually expanded its activities both on monitoring of atmospheric pollutants and on climate change.<sup>60</sup>

### ***CO<sub>2</sub> versus Aerosol***

By the late 1960s, it was widely accepted that the burning of fossil fuels was elevating the atmospheric CO<sub>2</sub> level. RCM simulations by Manabe and Wetherald convinced many, though not all, climatologists and meteorologists that this would contribute a warming effect to the earth’s surface temperature. The extent to which it would do so, however, was not yet clear. In addition, global temperatures were reported to have kept on falling.<sup>61</sup> This situation encouraged the examination of other possible causative agencies. Murray Mitchell and others had earlier looked into the cooling effects of volcanic dust.<sup>62</sup> In Britain, Hubert Lamb at the Met Office was conducting an extensive study on the subject.<sup>63</sup> As concerns grew about human capacity for influencing the environment, other scientists turned to anthropogenic aerosols. Like volcanic dust, they contended, particulate loading from human sources would backscatter incoming solar radiation, increasing planetary albedo and consequently cooling the earth.<sup>64</sup>

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57 Revelle was also present at the symposium. The proceedings of this symposium were published as an edited book in 1970. **S.F. Singer** (ed) *Global Effects of Environmental Pollution* (Dordrecht: D. Reidel, 1970)

58 Ibid. S. Fred Singer was an atmospheric physicist by training and was at the time deputy assistant secretary of the US Department of Interior.

59 **B. Bolin** *Changes of the Carbon Dioxide Content of the Atmosphere - a Global Air Pollution Problem* (Stockholm: Meteorological Institute, 1966)

60 **D.A. Davies** 'The Role of the WMO in Environmental Issues', *Int. Organ.* **26** (1972): 327-36.

61 **M.I. Budyko** 'Effect of Solar Radiation Variations on Climate of Earth', *Tellus* **21** (1969): 611-19; **J.M. Mitchell** 'A Preliminary Evaluation of Atmospheric Pollution as a Cause of the Global Temperature Fluctuation of the Past Century', in *Global Effects of Environmental Pollution*, S.F. Singer (ed) (Dordrecht: D. Reidel, 1970): 139-55.

62 **Callendar** 'Temperature Fluctuations and Trends over the Earth'; **W.J. Humphreys** *Physics of the Air*, 3<sup>rd</sup> edition (New York: McGraw-Hill, 1940); **Mitchell** 'Recent Secular Changes of Global Temperature'; **H. Wexler** 'Volcanoes and World Climate', *Sci. Am.* **186** (1952): 74-80. If these effects were assumed to be strong enough, the warming trend before 1940 would then be due to a reduced number of volcanic eruptions and ensuing clearance of dust in the atmosphere. This account was only partly successful in explaining the reported temperature changes.

63 **H.H. Lamb** 'Volcanic Dust in the Atmosphere - With a Chronology and Assessment of Its Meteorological Significance', *Philos. Tr. R. Soc. S.-A* **266** (1970): 425-533. See Chapter 3.

64 **R.A. McCormick and J.H. Ludwig** 'Climate Modification by Atmospheric Aerosols', *Science* **156** (1967):

Hence, the same environmental concerns that promoted the global warming hypothesis were now also giving weight to the counter hypothesis. The debate over CO<sub>2</sub>-induced climate change was quickly re-framed in terms of the competing climatic effects of CO<sub>2</sub> and aerosols, or in terms of global warming versus global cooling. One active proponent of the anthropogenic dust-induced cooling hypothesis was Reid Bryson, a geologist-turned-meteorologist at the University of Wisconsin-Madison. In a series of papers published during the late 1960s, he claimed that, amongst various factors affecting climate, the increase in atmospheric aerosols by human activities was “currently dominant” and was responsible for the sustained cooling since World War II.<sup>65</sup> Of the relative importance of CO<sub>2</sub> he was doubtful, and speculated that the rise of atmospheric CO<sub>2</sub> could be “the effect of increased temperature rather than its cause.”<sup>66</sup>

Certainly, not all scientists agreed. In 1968, Mitchell again evaluated causes of the global climatic trends, this time adding anthropogenic aerosols in his discussion.<sup>67</sup> Although he concurred with Bryson that increased dust loading might be responsible for the recent cooling trend, he argued that this was probably due more to volcanic eruptions than to human activities. As for the warming effect of CO<sub>2</sub>, Mitchell more or less adopted the results of Manabe and Wetherald. Of the two pollutants at present, he therefore concluded, CO<sub>2</sub> increase was “several times more influential” in affecting the global temperature than the human-derived dust loading. He acknowledged that the cooling effect of anthropogenic aerosols might in due course overtake the CO<sub>2</sub> effect because of their differing atmospheric residence times, but only after a global warming until 2000 AD and with the rate of industrial production unchanged at current levels.<sup>68</sup>

The reason for Mitchell’s doubt about the role of anthropogenic aerosols consisted in

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1358-59; **J.T. Peterson and R.A. Bryson** 'Atmospheric Aerosols - Increased Concentrations during the Last Decade', *Science* **162** (1968): 120-21.

65 **R.A. Bryson** "All Other Factors Being Constant ... " A Reconciliation of Several Theories of Climatic Change', *Weatherwise* **21** (1968): 56-61; **R.A. Bryson and D.A. Baerreis** 'Possibilities of Major Climatic Modification and Their Implications - Northwest India: A Case for Study', *B. Am. Meteorol. Soc.* **48** (1967): 136-42; **Peterson and Bryson** 'Atmospheric Aerosols'. Bryson also gave a presentation at the AAAS symposium. See **R.A. Bryson and W.M. Wendland** 'Climatic Effects of Atmospheric Pollution', in *Global Effects of Environmental Pollution*, S.F. Singer (ed) (Dordrecht: D. Reidel, 1970): 130-38.

66 **Bryson** "All Other Factors Being Constant ... " .

67 **Mitchell** 'A Preliminary Evaluation of Atmospheric Pollution' .

68 Mitchell certainly thought that human activities were becoming a major factor in determining the future climate. But he also maintained that the present climate change was predominantly natural in origin. He wrote, "... Other environmental agencies, presumably natural ones, are required to account for the main part of the observed fluctuation of world-average temperature during the past century, and will continue to exert an important influence on climate in the future." Ibid.

that the observed variations of anthropogenic dust loading were an order of magnitude less than those of volcanic dust loading.<sup>69</sup> Later, the dust-cooling hypothesis came to be criticized on the grounds that it underestimated the absorption properties of tropospheric aerosols. For example, Robert J. Charlson and Michael J. Pilat applied meteorologists at the University of Washington, suggested that, for certain anthropogenic aerosols, the net effect of both absorption and backscattering might well be a warming within and below an aerosol layer.<sup>70</sup>

The CO<sub>2</sub> versus Aerosol debate continued into the late 1960s and early 1970s. Some scientists believed that global warming was more likely to occur. As noted in Chapter 3, a Russian climatologist, Mikhail Budyko, who pioneered an energy balance model (EBM) for climate studies, stressed that the enhanced greenhouse effect by CO<sub>2</sub> increases could melt the Arctic ice caps.<sup>71</sup> In his paper describing an EBM of a different kind, William D. Sellers at Arizona wrote that “man’s activity, if it continues unabated, should eventually lead ... to a climate much warmer than today”.<sup>72</sup> Incorporating the work of Charlson and Pilat, Mitchell also asserted that anthropogenic aerosols might augment, not counteract, CO<sub>2</sub>-induced global warming.<sup>73</sup> By contrast, others thought that aerosols would have a bigger, or at least comparable, cooling effect. Influenced by the ongoing discussions about atmospheric pollution, S. Ichtiaque Rasool and his research associate Stephen H. Schneider at the NASA Goddard Institute of Space Studies (GISS) applied the radiation calculation skills developed in planetary science to the earth’s climate. In their model, a four-fold increase in aerosols, which was expected to materialize within less than 50 years, was sufficient to cool the earth by as much as 3.5 °C.<sup>74</sup> These results were supported qualitatively by Japanese meteorologists G. Yamamoto and M. Tanaka, who carried out

69 The total anthropogenic aerosol load was then estimated to be comparable to the average stratospheric dust load from volcanic eruptions. Ibid.

70 **R.J. Charlson and M.J. Pilat** 'Climate - The Influence of Aerosols', *J. Appl. Meteorol.* **8** (1969): 1001-02.

71 **Budyko** 'Effect of Solar Radiation Variations on Climate of Earth'.

72 **W.D. Sellers** 'A Global Climate Model Based on the Energy Balance of the Earth-Atmosphere System', *J. Appl. Meteorol.* **8** (1969): 392-400.

73 **J.M. Mitchell** 'The Effect of Atmospheric Aerosols on Climate with Special Reference to Temperature near the Earth's Surface', Ibid. **10** (1971): 703-14.

74 **S.I. Rasool and S.H. Schneider** 'Atmospheric Carbon Dioxide and Aerosols - Effects of Large Increases on Global Climate', *Science* **173** (1971): 138-41. This paper was followed by another series of debates. See **S.H. Schneider** 'A Comment on "Climate - The Influence of Aerosols"', *J. Appl. Meteorol.* **10** (1971): 840-41; **R.J. Charlson and M.J. Pilat** 'Reply', *J. Appl. Meteorol.* **10** (1971): 841-42; **R.J. Charlson et al.** 'Aerosol Concentrations - Effect on Planetary Temperatures', *Science* **175** (1972): 95-96. GISS was a division of NASA Goddard Space Flight Center's Earth Sciences Directorate.



similar calculations.<sup>75</sup>

Some attempts had been made to resolve this divergence of opinion, but were not successful. In 1971, shortly after Rasool and Schneider completed their calculations, the Study of Man's Impact on Climate (SMIC) was sponsored by MIT. This workshop, like the Study of Critical Environmental Problems (SCEP) held a year earlier, involved many of the leading experts in dynamical meteorology, atmospheric physics and chemistry, and physical oceanography, and examined a wide range of different human impacts on climate.<sup>76</sup> Obviously, emphases were placed upon the climatic effects of CO<sub>2</sub> and aerosols. On one evening during the SMIC, William W. Kellogg, one of its joint secretaries, organized an informal meeting in order to forge consensus on the combining effect of these two pollutants for the next decades. As Kellogg recalled, "the impasse prevailed" and consensus could not be reached.<sup>77</sup>

Yet this unresolved controversy was not as damaging to the development of global warming research as it might appear. While there was a substantial degree of uncertainty as to whether overall human activities would warm or cool the earth, the warming effect of increased CO<sub>2</sub> itself had rarely been contested throughout the discussions. GFDL's RCM simulations were influential in this respect. SCEP's CO<sub>2</sub> discussion relied primarily on the 1967 result by Manabe and Wetherald.<sup>78</sup> By the time SMIC was organized, Manabe's revised computation with a different radiative transfer scheme became available, and yet it reproduced the same conclusion (*i.e.*, a warming of *ca.* 2 °C for doubled CO<sub>2</sub>).<sup>79</sup> Rasool and Schneider, in their study noted above, came up with a lower figure of 0.8 °C for doubled CO<sub>2</sub>. However, they did not challenge the enhanced greenhouse effect *per se*. Their model also had more idealized background assumptions than Manabe's RCM.<sup>80</sup> In

75 **G. Yamamoto and M. Tanaka** 'Increase of Global Albedo Due to Air Pollution', *J. Atmos. Sci.* **29** (1972): 1405-12.

76 **SCEP** *Man's Impact on the Global Environment - Report of the Study of Critical Environmental Problems* (Cambridge: MIT Press, 1970); **SMIC** *Inadvertent Climate Modification - Report of the Study of Man's Impact on Climate* (Cambridge: MIT Press, 1971)

77 **W.W. Kellogg** 'Mankind's Impact on Climate - The Evolution of an Awareness', *Climatic Change* **10** (1987): 113-36.

78 **SCEP** *Man's Impact on the Global Environment*.

79 Manabe's new estimate for doubled CO<sub>2</sub> was 1.9 °C. **S. Manabe** 'Estimates of Future Change of Climate Due to the Increase of Carbon Dioxide Concentration in the Air', in *Man's Impact on the Climate*, W.H. Matthews *et al.* (eds) (Cambridge: MIT Press, 1971): 249-64.

80 As in Manabe's RCM, the 1-D planetary radiation balance model used by Rasool and Schneider used constant relative humidity and cloudiness. But they also fixed lapse rate and stratospheric temperature. *Lapse rate* refers to the rate of change of an atmospheric variable, temperature in this case, with height. **S.I. Rasool**

reviewing various studies on the climatic effects of atmospheric CO<sub>2</sub>, the SMIC report stated that GFDL's results "appear to be the most reliable ones."<sup>81</sup> At a more general level, the CO<sub>2</sub> versus Aerosol debate served to highlight the issue of anthropogenic climate change, which had been neglected by the atmospheric sciences communities. This helped bring more scientists into global warming research. The increasing use of physico-mathematical models was particularly important in attracting mainstream meteorologists and atmospheric scientists.

### Gradual Shift to Global Warming

Meanwhile, another type of environmental concern, in a broad sense, arose in the late 1960s to mid 1970s. A sequence of weather extremes such as cold winter and drought hit food production badly in many countries, most notably in the Soviet Union and the African Sahel. The cold winter and hot dry summer of 1972 forced the Soviet Union to purchase large amounts of grain from the US. This drastically reduced US reserves and led to dramatic fluctuations in world cereal prices.<sup>82</sup> In West Africa, in the 1973 Sahel drought alone, nearly 100,000 people were estimated to have died due to starvation and famine-related disease.<sup>83</sup> Weather anomalies such as these were perceived to threaten not only the political stability of third world countries but global food supplies, causing uneasiness among the political establishment and media of the western world, above all the US.<sup>84</sup>

The conventional wisdom had been that weather extremes were basically episodic natural disturbances and should be viewed as "part of climate".<sup>85</sup> Faced with an increase

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and **S.H. Schneider** 'Atmospheric Carbon Dioxide and Aerosols: Effects of Large Increases on Global Climate', *Science* **173** (1971): 138-41. **R.E. Newell** and **T.G. Dopplack** at MIT, in their study of radiative heating rates, also emphasized that the effects of changing CO<sub>2</sub> would be smaller than the influence of water vapour, but their work did not arouse much attention. **R.E. Newell** and **T.G. Dopplack** 'The Effect of Changing CO<sub>2</sub> Concentration on Radiative Heating Rates', *J. Appl. Meteorol.* **9** (1970): 958-59. Later in 1979, Newell and Dopplack published one of the first global warming sceptic papers. **R.E. Newell** and **T.G. Dopplack** 'Questions Concerning the Possible Influence of Anthropogenic CO<sub>2</sub> on Atmospheric Temperature', *J. Appl. Meteorol.* **18** (1979): 822-25.

81 **SMIC** *Inadvertent Climate Modification*.

82 **S.H. Schneider** and **L.E. Mesirow** *The Genesis Strategy - Climate and Global Survival* (New York: Plenum Press, 1976)

83 **H. Sheets** and **R. Morris** 'Disaster in the Desert', in *The Politics of Natural Disaster - The Case of the Sahel Drought*, M.H. Glantz (ed) (New York: Praeger, 1976): 25-76.

84 See also Chapter 3.

85 **H.E. Landsberg** 'Sahel Drought - Change of Climate or Part of Climate', *Arch. Meteor. Geophys. B.* **23** (1975):

in the frequency and intensity of weather extremes, more and more scientists came to believe that they might be associated with the enduring decline of world-wide temperatures.<sup>86</sup> Many climatologists held that this cooling trend was predominantly natural in origin – due to either long-term cyclic climate changes or volcanic dust, or the combination of both. Others kept drawing attention to human impact on climate. Bryson in particular put forward a controversial account for the Sahel drought. He argued that a cooling trend in the Northern Hemisphere shifted the monsoon belt away from the Sahara, reducing precipitation, but that a substantial part of that contributory cooling was caused by aerosols from human activities outside of the Sahel.<sup>87</sup>

Whether of natural or anthropogenic origin, the argument for prolonged global cooling and its potential hazards seemed to be gaining some popularity. A group of climatologists and geologists went on to suggest that the earth might be on the brink of transition from a warm interglacial climate to ice age conditions. Representatives of this group even wrote to President Nixon about its possible environmental repercussions.<sup>88</sup> Bryson also strongly warned of an imminent global food crisis based on his anthropogenic dust-induced cooling hypothesis.<sup>89</sup> Two CIA internal reports on climate problems prepared in 1974 quoted Bryson as a principal source of expertise.<sup>90</sup>

Discussions over global cooling, along with an already widespread concern about the large-scale effects of atmospheric pollution,<sup>91</sup> spurred an ever-growing recognition of climate change as one of the major research topics. However, the majority of meteorologists were more cautious in interpreting the climatic trend and predicting future climate change. Rather than being caught up in specific debates as to in which direction climate might change, or to what societal consequences it would bring about, the

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193-200.

86 **H.H. Lamb** *Climate: Present, Past and Future - Vol. 1 Fundamentals and Climate Now* (London: Methuen, 1972)

87 **R.A. Bryson** 'Drought in Sahelia - Who or What is to Blame?' *Ecologist* **3** (1973): 366-71.

88 **G.J. Kukla and R.K. Matthews** 'When Will the Present Interglacial End?' *Science* **178** (1972): 190-1. See also Chapter 3.

89 **J. Gribbin** 'Weather Warning - You Are Now Experiencing a Climatic Change', *Nature* **252** (1974): 182-83.

90 These reports were released to the public in 1976. **S.R. Weart** 'The Public and Climate', in *The Discovery of Global Warming*, <http://www.aip.org/history/climate> (recorded copy at American Institute of Physics, 2001): p. Public-15 of 08/01/01 text. For the content of CIA reports, see **Impact Team** *The Weather Conspiracy - The Coming of the New Ice Age* (New York: Ballantine Books, 1977). See also Chapter 3, note 146.

91 For instance, the potential climatic effects of SSTs continued to evoke controversy in the early 1970s. This led the US Department of Transportation and the Met Office to each conduct a three-year climate impact assessment. See Chapter 3.

meteorological community used the political and media attention as an opportunity to promote the broad field of climate research. In 1974, the Global Atmospheric Research Programme (GARP), an international scientific collaboration jointly sponsored by the WMO and ICSU, convened a study conference on the “Physical Basis of Climate and Climate Modelling” in Stockholm. The conference report stated:

[S]ome have predicted that the climate will become warmer, while others have stated that the cooling trend which began in the 1940's will continue for several decades. A view more widely accepted by specialists is that our understanding of climate and climate variability is far too meagre to warrant pronouncements of this sort. The important task at this time is rather to give an accurate picture of what we know and what we don't know and to develop a programme of advancing our knowledge.<sup>92</sup>

Later in early 1975, after a series of meetings, the NAS published a report *Understanding Climatic Change: A Program for Action*. Prepared by the Panel on Climatic Variation of the US Committee for GARP (USC-GARP), the report followed the same line of reasoning and called for the expansion of climate research.<sup>93</sup>

The changing focus of climate debates from global cooling to climate change in general was propitious for the development of global warming research. Although both the GARP study conference and the NAS report adopted an equally cautious stance on CO<sub>2</sub>-induced global warming, they recognized it as one of the most important issues concerning anthropogenic climate change.<sup>94</sup> Global warming research also benefited in a different way. These events signalled the transition to a new GCM-based science of global climate change.<sup>95</sup> The PSAC, SCEP and SMIC all recommended the development of GCM for climate research, but only since the mid 1970s did GCMs become available for extensive use in the study of climate change. Mainstream meteorologists were enthusiastic about the prospect of this new venture.<sup>96</sup> Quite naturally, when Manabe first presented his GCM

92 **GARP** *The Physical Basis of Climate and Climate Modelling - Report of the International Study Conference*, GARP Publication Series, No. 16 (Geneva: WMO, 1975)

93 **USC-GARP** *Understanding Climatic Change - A Program for Action* (Washington, DC: National Academy of Sciences, 1975)

94 In fact, many of the scientists who had been involved in global warming research took an active part. The chairman of the GARP study conference was Bert Bolin. The conference participants included Eriksson, Keeling, Manabe, Mitchell, Möller, Schneider and Smagorinsky. The USC-GARP and its Panel on Climatic Variation also involved Manabe, Mitchell and Smagorinsky. **GARP** *The Physical Basis of Climate and Climate Modelling*.

95 See Chapter 3.

96 **A. Kasahara** *Interview by the author* (May 9, 2001)

simulation on the climatic effects of CO<sub>2</sub> at the GARP study conference, his results sparked considerable interest. Using a 9-level GCM, Manabe calculated that doubled CO<sub>2</sub> would raise the annual mean temperature by 2.9 °C, somewhat higher than his previous estimate, with the increased intensity of the hydrological cycle.<sup>97</sup> Many of those who suspected the role of CO<sub>2</sub> as an effective climate forcing at the time based their criticisms on inductive reasoning.<sup>98</sup> In the eyes of mainstream meteorologists, Manabe's GCM results were more attractive.

While Manabe's GCM study was pivotal in directing attention to global warming, some others pursued the studies using simple climate models. For instance, Sellers applied a modified version of his EBM to the CO<sub>2</sub> problem.<sup>99</sup> V. Ramanathan at the NASA Langley Research Center further improved Manabe and Wetherald's 1-D RCM and used it for CO<sub>2</sub>-doubling simulations.<sup>100</sup> Despite the differences in their models, they produced temperature sensitivity values within the range of 1.3–1.5 °C for doubled CO<sub>2</sub>, bolstering the global warming hypothesis. In late 1975, Schneider, now at NCAR, compared various modelling studies and concluded that the "state-of-the-art order-of-magnitude estimate" for the global surface temperature increase from a doubling of CO<sub>2</sub> was between 1.5 and 3 °C.<sup>101</sup>

A series of other events also pointed to global warming. First, the net cooling effect of anthropogenic aerosols continued to be questioned. The natural behaviour of the particulate matter was still poorly understood and insufficiently monitored.<sup>102</sup> But

97 **S. Manabe** 'The Use of Comprehensive General Circulation Modelling for Studies of the Climate and Climate Variation', in *The Physical Basis of Climate and Climate Modelling* (Geneva: WMO, 1975): 148-62. A more complete paper version of his presentation was published in early 1975. **S. Manabe and R.T. Wetherald** 'Effects of Doubling CO<sub>2</sub> Concentration on Climate of a General Circulation Model', *J. Atmos. Sci.* **32** (1975): 3-15. Manabe's GCM still had many simplifications – his model had idealized geography, fixed clouds and swamp ocean. Actually, this simulation was not global but confined to the Northern Hemisphere.

98 For instance, in his paper published in 1974, Bryson argued that if CO<sub>2</sub> was a dominant factor "the mean global surface temperature should have risen steadily and smoothly, at an increase rate, by about 0.25 °C". But, he continued, "[i]t has not, and in fact has decreased by about that amount since 1940. There clearly are other factors of greater magnitude that dominate climatic change in terms of mean global surface temperature." **R.A. Bryson** 'A Perspective on Climatic Change', *Science* **184** (1974): 753-60. See below.

99 Sellers' model was a 2-D EBM with fixed relative humidity, lapse rate and cloudiness. It nevertheless incorporated feedback from snow and ice cover. See **W.D. Sellers** 'Reassessment of Effect of CO<sub>2</sub> Variations on a Simple Global Climatic Model', *J. Appl. Meteorol.* **13** (1974): 831-33.

100 Ramanathan's RCM was basically the same as that developed by Manabe and Wetherald, but with different infrared and solar radiation schemes. **V. Ramanathan** 'A Study of the Sensitivity of Radiative-Convective Models', in *Second Conference on Atmospheric Radiation* (Boston: American Meteorological Society, 1975): 124-25.

101 **S.H. Schneider** 'On the Carbon Dioxide-Climate Confusion', *J. Atmos. Sci.* **32** (1975): 2060-66.

102 **B. Bolin et al.** 'Stockholm Tropospheric Aerosol Seminar - Measurement of Regional to Global-scale Pollution by Airborne Particles', *B. Am. Meteorol. Soc.* **55** (1974): 228-31.



Schneider, Kellogg and his co-workers at NCAR proposed that, whereas the net effect of tropospheric aerosol increase would be a cooling over the oceans, it would be a warming over land where most of the aerosols from industrial and agricultural activities were found.<sup>103</sup> Secondly, the global cooling trend was no longer certain. In 1975, James K. Angell and J. Korshover at the National Oceanic and Atmospheric Administration (NOAA) reported that between 1963 and 1973 there was a slight cooling in the Northern Hemisphere, but that it had been compensated by an equivalent warming in the Southern Hemisphere. Thus, they wrote, “at this time, we can conceive of no way of foretelling the global temperature change which will occur during the next few decades, or the next few centuries”.<sup>104</sup> Finally, it was suggested that anthropogenic trace chemicals other than CO<sub>2</sub> could also act as greenhouse gases, accelerating global warming. The controversy over the effects of chlorofluorocarbons (CFCs) on stratospheric ozone<sup>105</sup> prompted Ramanathan to examine its radiation properties using his RCM. His calculations showed that an increase in atmospheric concentrations of these compounds might lead to an appreciable rise in the global surface temperature.<sup>106</sup>

Some of these findings, including the modelling results, were contested. Many uncertainties remained to be resolved. Nevertheless, from around this time onwards, a shift in thinking toward global warming gradually began to emerge.

### British Lack of Interest in Global Warming

The acceptance of CO<sub>2</sub>-induced global warming as an important research agenda was a slower process in Britain. This is interesting since it was a British scientist, Guy Callendar, who played a crucial role in reviving the CO<sub>2</sub> theory of climate change in the early to mid 20<sup>th</sup> century. Callendar was a steam engineer, and his amateur status as a scientist might have had a negative influence on how his research had been received by the mainstream

103 **S.H. Schneider and W.W. Kellogg** 'The Chemical Basis for Climate Change', in *Chemistry of the Lower Atmosphere*, S.I. Rasool and R.D. Cadle (eds) (New York: Plenum Press, 1973): 203-49; **W.W. Kellogg et al.** 'Effect of Anthropogenic Aerosols on the Global Climate', in *Proceedings of the WMO/IAMAP Symposium on Long-term Climatic Fluctuations*, WMO No. 421 (Geneva: WMO, 1975): 323-30.

104 **J.K. Angell and J. Korshover** 'Estimate of Global Change in Tropospheric Temperature between 1958 and 1973', *Mon. Weather Rev.* **103** (1975): 1007-12.

105 **M.J. Molina and F.S. Rowland** 'Stratospheric Sink for Chlorofluoromethanes - Chlorine-Atom Catalyzed Destruction of Ozone', *Nature* **249** (1974): 810-12.

106 **V. Ramanathan** 'Greenhouse Effect Due to Chlorofluorocarbons - Climatic Implications', *Science* **190**

atmospheric science community.<sup>107</sup> It should be noted, though, that Callendar's work was not simply ignored. His results were often presented to the Royal Meteorological Society (RMS) meetings, and his contribution to atmospheric radiation studies was praised by prominent meteorologists David Brunt and Sydney Chapman.<sup>108</sup>

Whether or not Callendar was respected in the British meteorological community, investigations into the radiative properties of trace gases such as CO<sub>2</sub>, which constituted the basis for greenhouse research, were taken seriously. Just as elsewhere, they were seen as integral to an understanding of the earth's atmosphere. In addition, from early on, British atmospheric physicists well understood the basic concept of the enhanced greenhouse effect, independently of Callendar's research. In the early 1940s, Richard Goody, who later wrote a seminal book *Atmospheric Radiation: Theoretical Basis*,<sup>109</sup> was working as a wartime meteorologist at the Aircraft and Armament Experimental Establishment. His task was to conduct ambient atmospheric measurements, but he also assisted Oxford atmospheric physicists Gordon Dobson and Alan Brewer in measuring water vapour in the stratosphere. One day, Brewer gave him a report by Chaim Pekeris on the historical development of atmospheric radiative equilibrium calculations. This report, which inspired Goody's future research, contained Arrhenius' work on the climatic effects of increased CO<sub>2</sub>.<sup>110</sup>

Nonetheless, apart from Callendar, no British scientist really took up the issue of CO<sub>2</sub>-induced climate change in the 1950s and early 1960s. The results of Callendar or Plass were discussed from time to time, yet the exchanges were limited to particular radiation issues, and seldom addressed the question of climate.<sup>111</sup> This was partly accidental. British atmospheric physicists, who could have developed their interest in CO<sub>2</sub>-induced global warming, were busy working on other topics at that time. The Oxford group led by

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(1975): 50-52.

107 **Weart** 'Global Warming, Cold War, and the Evolution of Research Plans'.

108 **J.R. Fleming** *Historical Perspectives on Climate Change* (Oxford: Oxford University Press, 1998)

109 **R.M. Goody** *Atmospheric Radiation - Theoretical Basis*, 1<sup>st</sup> edition (Oxford: Oxford University Press, 1964)

110 **R.M. Goody** *Interview by the author* (January 12, 2001); 'Observing and Thinking about the Atmosphere', *Annu. Rev. Energ. Env.* **27** (2002): 1-20. For Pekeris' report, see **C.L. Pekeris** *The Development and Present Status of the Theory of the Heat Balance in the Atmosphere*, MIT Meteorology Course, No. 5 (Cambridge: MIT, 1932). In 1934, Pekeris received a PhD dissertation on the same topic from MIT's Department of Meteorology. However, he soon moved to a rather different field of geophysics and then to theoretical physics.

111 **R.M. Goody and G.D. Robinson** 'Radiation in the Troposphere and Lower Stratosphere', *Q. J. Roy. Meteor. Soc.* **77** (1951): 151-87; **G.N. Plass et al.** 'Discussions', *Q. J. Roy. Meteor. Soc.* **83** (1957): 272-75.

Dobson and Brewer adhered to the studies of stratospheric constituents, mainly ozone.<sup>112</sup> Goody, first at Cambridge and then at Imperial, had carried out research into various aspects of atmospheric radiation.<sup>113</sup> These radiation studies had relevance to research on global physical climate. Yet Goody did not pursue his research in that direction, and left for Harvard in 1958. In the Met Office, among others, Robert J. Murgatroyd and George D. Robinson had the skills and knowledge to examine the enhanced greenhouse effect by CO<sub>2</sub>. On the other hand, their research centred upon the high atmosphere and the measurement of atmospheric radiation, respectively.<sup>114</sup>

The lack of interest in CO<sub>2</sub>-induced global warming was also to do with the fact that much of the manpower and resources in British meteorological research was concentrated in the Met Office, whose chief research concern was the development and improvement of weather forecasting. In contrast to the US, there were not many academic meteorologists in Britain. Until the mid 1960s, when Edinburgh and Reading established their own departments, only Imperial had an independent academic unit in meteorology.<sup>115</sup> Even these few academic meteorologists were “dedicated to details of the time-dependent atmosphere”, and were just “politely tolerant of less important questions about such subjects as the physical climate”,<sup>116</sup> not to mention atmospheric chemistry. Most atmospheric physicists were affiliated with the physics departments. While this by no means prevented them from communicating with the wider meteorological community, they did not actively seek to link their radiation work to the earth’s climate. Goody reminisced:

[The climatological implications of radiation studies] were of less interest in those days to the weather community. The weather community recognized that radiation had

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112 **G.M.B. Dobson** 'Forty Years' Research on Atmospheric Ozone at Oxford - A History', *Appl. Optics* **7** (1968): 387-405.

113 **Goody** 'Observing and Thinking about the Atmosphere'. However, he occasionally tackled the problem of other planetary atmospheres. See, e.g., **J. Grandjean and R.M. Goody** 'The Concentration of Carbon Dioxide in the Atmosphere of Mars', *Astrophys. J.* **121** (1955): 548-52.

114 **R.J. Murgatroyd and F. Singleton** 'Possible Meridional Circulations in the Stratosphere and Mesosphere', *Q. J. Roy. Meteor. Soc.* **87** (1961): 125-35. **G.D. Robinson** 'The Use of Surface Observations to Estimate the Local Energy Balance of the Atmosphere', *Proc. R. Soc. Lon. Ser.-A* **236** (1956): 160-71. Robinson later emigrated to the US, and played an important role in the SCEP and SMIC workshops. **G.D. Robinson** Interview by E. Droessler (AMS Tape Recorded Interview Project: June 27-28, 1994)

115 **R.C. Sutcliffe** 'Meteorology in British Universities', *Weather* **20** (1965): 208-11. See also Chapter 2.

116 **Goody** 'Observing and Thinking about the Atmosphere'. Sir George Simpson, former director of the Met Office, was perhaps one of the very few meteorologists who attempted to link radiation studies to the discussion of climate. But his work on terrestrial radiation was more of a spare time research. **G.C. Simpson** 'Further Studies in World Climate', *Q. J. Roy. Meteor. Soc.* **83** (1957): 459-85. See also Chapter 2.

some role in long-range forecasts, but it was a secondary consideration, and a physical climate community scarcely existed at the time.<sup>117</sup>

Given this situation, it is perhaps not surprising that concerns over nuclear fallout and other atmospheric pollution during the late 1950s to mid 1960s did not draw the interest of the meteorological community to human impact on climate in Britain, as they did in other countries. Not only that, British meteorologists were unconvinced of the very idea that human activities could significantly alter the earth's radiation balance, through albedo or atmospheric changes, thereby modifying climate.<sup>118</sup> The studies of the effects of pollution on the atmosphere, when carried out, were directed towards the local transport and dispersion of atmospheric pollutants – more familiar terrain for meteorologists.<sup>119</sup> Nor did these concerns stimulate British physical oceanographers or geochemists to launch research into carbon exchanges between the atmosphere and the ocean. For example, a geochemist Alan Walton had done some research into the global radiocarbon cycle during his post-doc with Wallace S. Broecker at Columbia's Lamont Geological Observatory in the US.<sup>120</sup> After returning to the UK, he moved away from the topic and did not recommence this type of research until the early 1970s.

Some members of the meteorological community, *i.e.*, observational climatologists, did study climate and its change. Lamb was particularly instrumental in fostering the empirical studies of world-wide climate change within historical time-scales.<sup>121</sup> Unlike US climatologists Murray Mitchell and Helmut Landsberg, however, Lamb and other British climatologists did not couch their studies in the language of physical or dynamical meteorology, and did not receive much respect from mainstream meteorologists.<sup>122</sup> Their

117 **Goody** 'Observing and Thinking about the Atmosphere'.

118 See, *e.g.*, **G. Sutton** 'Man's Attempts to Control the Weather', *New Sci.* (September 4, 1958): 744-46. See also Chapters 2 and 3.

119 **J. Crabtree** 'The Travel and Diffusion of the Radioactive Material Emitted during the Windscale Accident', *Q. J. Roy. Meteor. Soc.* **85** (1959): 362-70; **P.J. Meade** 'Atmospheric Pollution - Its Origins and Prevention', *Meteorol. Mag.* **94** (1965): 375-76.

120 **W.S. Broecker and A. Walton** 'Radiocarbon from Nuclear Tests', *Science* **130** (1959): 309-14; 'The Geochemistry of C-14 in Fresh-Water Systems', *Geochim. Cosmochim. Ac.* **16** (1959): 15-38.

121 **H.H. Lamb and A.I. Johnson** 'Climatic Variation and Observed Changes in the General Circulation', *Geogr. Ann. A.* **41** (1959): 94-134; **H.H. Lamb** 'Climatic Change within Historical Time as Seen in Circulation Maps and Diagrams', *Ann. NY Acad. Sci.* **95/1** (1961): 124-61; 'Climate in the 1960s - Changes in World's Wind Circulation Reflected in Prevailing Temperatures, Rainfall Patterns and Levels of African Lakes', *Geogr. J.* **132** (1966): 183-212. For other early British climatological studies, see **D.J. Schove** 'The Climatic Fluctuation since AD 1850 in Europe and the Atlantic', *Q. J. Roy. Meteor. Soc.* **76** (1950): 147-65; **G. Manley** 'Reviews of Modern Meteorology - 9. Climatic Variation', *Q. J. Roy. Meteor. Soc.* **79** (1953): 185-209; 'The Mean Temperature of Central England, 1698-1952', *Q. J. Roy. Meteor. Soc.* **79** (1953): 242-61.

122 See Chapter 3.

studies were also almost exclusively concerned with natural climate changes. Although the examination of external climate forcings was often extended to include anthropogenic factors, human impact on climate was never dealt with in detail.<sup>123</sup> Even when the global warming hypothesis was discussed, strong reservation was expressed. In his book published in 1972, Lamb offered a similar argument to Bryson's:

Since observation shows that the average surface temperature over the Earth has nevertheless gone down by a few tenths of a degree between 1950 and the 1960s, it is clear that other agencies have had a more powerful effect than the carbon dioxide.<sup>124</sup>

After Callendar died in 1964,<sup>125</sup> there seemed to be no other proponents of the global warming hypothesis in Britain. Many British meteorologists remained sceptical of the possibility of large-scale climate modifications, deliberate or inadvertent.<sup>126</sup> The breach had to come from outside. What helped raise the British interest in CO<sub>2</sub>-induced global warming was a series of US modelling studies on inadvertent climate modifications.<sup>127</sup> Regardless of how the results were finally evaluated, the methods used by US scientists, especially the use of elaborate physico-mathematical models by Smagorinsky's group at GFDL, were highly valued by British meteorologists.<sup>128</sup> Slowly, the issues of human impact on climate, including CO<sub>2</sub>-induced global warming, attracted some attention. The annual report of the Met Office in 1968 stated:

It is realized that man's ability to modify the earth's surface and discharge artificial substances into the atmosphere is growing to the extent that the climate of the earth might be affected accidentally or, possibly, by design. It is essential therefore that an understanding of the atmosphere should be acquired from which the extent of any possible modifications can be assessed quantitatively prior to the event.<sup>129</sup>

This recognition of the need for studying human impact on climate was not immediately

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123 For instance, Lamb briefly mentioned the work of Plass in **Lamb and Johnson** 'Climatic Variation and Observed Changes in the General Circulation'. Later, Lamb began to talk about anthropogenic climate change more often, but in the context of emphasizing the importance of observational studies of climate and its change. See, e.g., **H.H. Lamb** 'Climatic Variation and Our Environment Today and in the Coming Years', *Weather* **25** (1970): 447-55. See also Chapter 3.

124 **Lamb** *Climate: Present, Past and Future - Vol. 1*, at 439.

125 **Anon.** 'Obituary - G. S. Callendar', *Q. J. Roy. Meteor. Soc.* **91** (1965): 112.

126 See Chapter 3.

127 For some of these early modelling works, see **US NRC** *Weather and Climate Modification*, Vol. 2.

128 **R.S. Harwood** *Interview by the author* (February 25, 1999)

129 **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1968)



translated into action. The Met Office scientists firmly believed that a quantitative understanding of such changes could only be achieved by using full, dynamical atmospheric models, *i.e.*, GCMs. Emphasis on GCMs was also present in the US, but was stronger in Britain. By the mid to late 1960s, the development of GCMs was under way in the Met Office.<sup>130</sup> In the course of these efforts, in collaboration with atmospheric physicists Clive Rodgers and Charles D. Walshaw at Oxford, the Met Office scientists formulated a radiative transfer scheme similar to Manabe's RCM.<sup>131</sup> However, as has been suggested, they did not use this scheme to study the climatic effects of increased CO<sub>2</sub> or any other issues of anthropogenic climate change.<sup>132</sup>

As world-wide environmental concern grew, CO<sub>2</sub>-induced global warming began to be discussed more often in Britain, but without much impact on the view of mainstream meteorologists and atmospheric physicists. In 1971, as Chapter 3 described, the Royal Commission on Environmental Pollution (RCEP) published the first official report. It had a short chapter, "Global Effects of Atmospheric Pollution", which was written with the help of the Met Office. The Office scientists were then aware of the CO<sub>2</sub> issue, largely through John Sawyer, director of research. Sawyer was a member of the Joint Organizing Committee for GARP and chairman of the WMO's Commission for Atmospheric Sciences, and through them, had many chances to review recent US studies on the greenhouse effect.<sup>133</sup> Relying on these US studies, the chapter concluded that a doubling of the atmospheric CO<sub>2</sub> content would increase the earth's surface temperature by *c.a.* 1.3 °C. The chapter nonetheless stated that the figures "are tentative, and cannot become more precise until more advanced mathematical models of the problem have been developed", emphasizing the importance of the role of GCMs.<sup>134</sup> Sawyer repeated the tone of the RCEP report in his short review of the greenhouse effect in 1972:

Although there maybe no immediate cause for alarm about the consequences of carbon

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130 See Chapter 2.

131 **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1965-70).

132 In fact, this scheme was more sophisticated than Manabe's, and was later used by Manabe's 1971 RCM study of the greenhouse effect in which an estimate of 1.9 °C was obtained for doubled CO<sub>2</sub>. See **Manabe** 'Estimates of Future Change of Climate Due to the Increase of CO<sub>2</sub>'.

133 Sawyer presented his review on the subject to the British meteorological community. See **J.S. Sawyer** 'Possible Effects of Human Activity on World Climate', *Weather* **26** (1971): 251-62; 'Man-made Carbon-Dioxide and the "Greenhouse" Effect', *Nature* **239** (1972): 23-26.

134 **RCEP** 'Chapter V. Global Effects of Atmospheric Pollution', in *First Report of the Royal Commission on Environmental Pollution* (London: HMSO, 1971): 36-43.

dioxide increase in the atmosphere, there is certainly need for further study. We need a better assessment ... [T]he development of increasingly sophisticated numerical simulation of the global climate seems the only possible approach.<sup>135</sup>

For British scientists, GCMs had to be constructed first if CO<sub>2</sub>-induced global warming were to be studied.

### Global Warming as an Energy Problem

By the time that the first GCM simulation of CO<sub>2</sub>-doubling was reported by Manabe in 1974, the Met Office had developed a comparably sophisticated, 5-level GCM.<sup>136</sup> This did not, however, directly lead to the commencement of global warming research. The Met Office's first and foremost institutional goal was weather forecasting, and its research priority was given to the improvement of operational numerical weather prediction models. The Met.O.20's GCM-based climate research had to be fitted into available computing resources left unused. The Met Office by then had a quite powerful IBM 360/195 computer, but climate research was still under heavy computing constraints.<sup>137</sup> A cautious style of climate modelling also contributed to delaying global warming research. For the Met Office scientists, their GCM was not yet suitable for full-fledged climate change simulations. They thought, for instance, that its radiative transfer scheme was inappropriate for CO<sub>2</sub>-doubling experiments<sup>138</sup> – even though it was actually more elaborate than the scheme Manabe used for his 1974 GCM study, which they respected.<sup>139</sup>

There was another reason for the Met Office's reluctance to get engaged in global warming research. Many British scientists at the time assumed that nuclear power would be the dominant form of energy in the near future. Concerns over nuclear safety and related environmental issues were beginning to be expressed, but more slowly than in

135 **Sawyer** 'Man-made Carbon-Dioxide and the "Greenhouse" Effect'.

136 **A. Gilchrist et al.** 'A Numerical Experiment Using a General Circulation Model of the Atmosphere', *Q. J. Roy. Meteor. Soc.* **99** (1973): 2-34. See also Chapters 2 and 3.

137 For a brief history of the Met Office's computing, see **M.K. Hinds** 'Computer Story', *Meteorol. Mag.* **110** (1981): 69-81.

138 **A. Gilchrist** *Interview by the author* (June 9, 1999)

139 The radiative transfer scheme that Manabe used for his 1974 GCM simulation was basically the same as that incorporated in the 1967 RCM study by Manabe and Wetherald. This was less sophisticated than a Rodgers-Walshaw formulation, which Manabe adopted in his 1971 RCM study. As noted above, this formulation was originally developed for the Met Office GCM. For the Met Office's cautious style of modelling, see also Chapters 2 and 3.

other countries.<sup>140</sup> The “oil shock” of 1973-74 tended to strengthen nuclear optimism. In 1976, John Mason, director of the Met Office, reviewed the state of climate modelling research. Quoting the GCM study of Manabe and Wetherald, he suggested that “an increase in CO<sub>2</sub> should produce higher temperatures by ‘the greenhouse effect’”, and yet implied that the predicted warming of 3 °C due to doubled CO<sub>2</sub> might not be threatening because “[w]ithin the next 50 years, fossil fuels will probably be largely exhausted and replaced by nuclear power stations”.<sup>141</sup> It was considered to be more important to examine the potential impact of large amounts of heat that these nuclear power stations would emit into the atmosphere. Hence, while the Met Office scientists did not embark on the study of CO<sub>2</sub>-induced global warming, they conducted GCM investigations into the release of waste heat.<sup>142</sup>

Many US scientists, at least initially, shared the view that a transition to nuclear economy would eventually take place. Rasool and Schneider wrote in 1971 that, by the time the increase in atmospheric pollution would reach the level of affecting global climate, “nuclear power may have largely replaced fossil fuels as a means of energy production”.<sup>143</sup> But the oil crisis and subsequent energy debates influenced climate research somewhat differently in the US. Anti-nuclear sentiments emerged earlier and were stronger there, addressing a range of issues related to the safety of nuclear power plants, nuclear weapons proliferation, and radioactive waste disposal. As in Britain, there were scientists who believed that CO<sub>2</sub>-induced global warming “could be averted by a more rapid than expected phase-out of fossil fuels as a prime energy source”.<sup>144</sup> Overall, with nuclear future in question, many other scientists felt that the climatic effects of increasing CO<sub>2</sub> became an even more important issue. In 1974, the NAS’ Geophysics

140 **W. Rudig** ‘Maintaining a Low Profile - The Anti-Nuclear Movement and the British State’, in *States and Anti-Nuclear Movements*, H. Flam (ed) (Edinburgh: Edinburgh University Press, 1994): 70-100.

141 **B.J. Mason** ‘Towards the Understanding and Prediction of Climatic Variations’, *Q. J. Roy. Meteor. Soc.* **102** (1976): 473-98. This paper was a written version of the Royal Meteorological Society’s Symons Memorial Lecture delivered in March 1976.

142 **A. Gilchrist** *Some Results from the UK Meteorological Office Model Relevant to the Waste Heat Experiments*, Met.O.20 Technical Note, No. II/59 (Bracknell: British Met Office, 1975). This study was eventually developed into a joint research project with IIASA. See **A.H. Murphy et al.** *The Impact of Waste Heat Release on Simulated Global Climate*, RM-76-079 (Laxenburg: IIASA, 1976); **J. Williams et al.** *Further Studies of the Impact of Waste Heat Release on Simulated Global Climate: Part I*, RM-77-15 (Laxenburg: IIASA, 1977); *Further Studies of the Impact of Waste Heat Release on Simulated Global Climate: Part II*, RM-77-034 (Laxenburg: IIASA, 1977).

143 **Rasool and Schneider** ‘Atmospheric Carbon Dioxide and Aerosols’.

144 **K.P. Beltzner** (ed) *Living with Climatic Change - Proceedings, Toronto Conference Workshop* (Ottawa: Science Council of Canada, 1976). This conference was co-sponsored by the American Meteorological Society.

Research Board set up the Panel on Energy and Climate. The Panel, whose chairman was Revelle, convened a symposium at an American Geophysical Union (AGU) meeting in the same year. CO<sub>2</sub>-induced global warming was the central topic.<sup>145</sup>

Thus, in the US, energy concerns added a new dimension to discussions over global warming. The increase in atmospheric CO<sub>2</sub> was no longer just a global pollution problem. It was also posing “climatic barriers to long-term energy growth”.<sup>146</sup> The government energy establishment, as expected, became interested in the CO<sub>2</sub> issue. In 1974, Alvin Weinberg, director of the Energy Research and Development Office of the Federal Energy Administration (FEA, one of the predecessors of the US Department of Energy) and former director of the Oak Ridge National Laboratory (ORNL), published a letter in *Science*, calling the attention of climate scientists to the global effects of man’s production of energy.<sup>147</sup> He also advised the Energy Research and Development Administration (ERDA, another predecessor of the US Department of Energy) to instigate a study programme into the long-term climatological effects of energy production. In the next year, the ERDA gave the responsibility of assessing the impacts of increasing CO<sub>2</sub> to ORNL’s newly established Institute of Energy Analysis (IEA) and appointed Weinberg its first director.<sup>148</sup>

Weinberg was a strong advocate of “nuclear energy revolution”.<sup>149</sup> Keeling later recalled that Weinberg did not hide his pro-nuclear view.<sup>150</sup> If the climatic consequences of burning fossil fuels turned out to be serious, Weinberg conceived, nuclear power might become a more tolerable energy alternative. This is not to say that he merely used the issue of CO<sub>2</sub>-induced global warming as an excuse for running a pro-nuclear campaign. Weinberg did not want to jump to any conclusions before the science was properly done, and was keen to provide an opportunity for climate scientists to advance global warming

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and the Science Council of Canada.

145 The Panel members included Keeling, Landsberg, Mitchell, Robinson, Smagorinsky, and others. The revised versions of the papers presented at this symposium were later published in the NAS report “Energy and Climate”. See **US NRC** *Energy and Climate* (Washington, DC: National Academy of Sciences, 1977)

146 **S.H. Schneider and R.D. Dennett** ‘Climatic Barriers to Long-term Energy Growth’, *Ambio* **4** (1975): 65-74.

147 **A.M. Weinberg** ‘Global Effects of Man’s Production of Energy’, *Science* **186** (1974): 205.

148 **Anon.** ‘Tribute to Alvin Weinberg’, *Oak Ridge National Laboratory Review* **28** (1995)

149 **A.M. Weinberg and G. Young** ‘Nuclear Energy Revolution - 1966’, *P. Natl. Acad. Sci. USA* **57** (1967): 1-15.

Weinberg earned his PhD in nuclear physics from the University of Chicago in 1939. He worked on the Manhattan Project from 1941-45. After various posts, Weinberg became director of the Oak Ridge National Laboratory (ORNL) in 1955, a position he held for 20 years.

150 **Keeling** ‘Rewards and Penalties’.

research. He was also supportive of the study of possible negative side effects of nuclear energy.<sup>151</sup> It was he who proposed the first GCM study of waste heat release. In the late 1960s, then as director of ORNL, he invited Warren Washington of NCAR to perform GCM simulations on that issue.<sup>152</sup>

ORNL published its first CO<sub>2</sub> report *The Global Carbon Dioxide Problem* in 1976, in consultation with Keeling, Manabe, Mitchell, Ramanathan and others.<sup>153</sup> The report did not present any new scientific findings, summarizing existing research. What it did was to clearly frame CO<sub>2</sub>-induced global warming as an energy issue. The report pointed out:

On the one hand, if the CO<sub>2</sub> problem turns out to be minor or insignificant, a large impediment to the accelerated and unlimited use of coal and oil shale will have been removed. If, on the other hand, continued increased addition of CO<sub>2</sub> to the atmosphere is early shown to be a threat, early de-emphasis of coal use, accelerated implementation of nuclear (particularly breeder) energy, speeded development of fusion, solar and other renewable forms of energy, and at least increased interim energy conservation must all receive high priority.<sup>154</sup>

In order to get an answer to this vital energy question, the report insisted, it was essential to organize more research into 1) the carbon cycle; 2) climate monitoring and modelling; and 3) the impact of climate change.

In the meantime, several new studies came out, reinforcing the global warming hypothesis. A more sophisticated RCM study conducted by T. Augustsson and V. Ramanathan computed an estimate of *ca.* 2–3 °C warming for doubled CO<sub>2</sub>.<sup>155</sup> A research group at the NASA GISS, led by James Hansen, argued that nitrous oxide, methane and ammonia, in addition to CO<sub>2</sub>, water vapour and CFCs, could act as efficient greenhouse

151 Weinberg was quite open to opposing views. While he was director of ORNL, he had several anti-nuclear staff members, one of whom was Claire Nader, sister of a consumer advocate Ralph Nader. See **F.J. Dyson** 'The Science and Politics of Climate', *Physics and Society* **29** (2000)

152 **W.M. Washington** Interview by P.N. Edwards (AMS Tape Recorded Interview Project: October 28, 1998). The results were presented to the SCEPT workshop in 1970. See **W.M. Washington** 'On the Possible Uses of Global Atmospheric Models for the Study of Air and Thermal Pollution', in *Man's Impact on the Climate*, W.H. Matthews *et al.* (eds) (Cambridge: MIT Press, 1971): 265–76.

153 **C.F. Baes, Jr. et al.** *The Global Carbon Dioxide Problem*, ORNL-5194 (Oak Ridge: ORNL, 1976)

154 *Ibid.*

155 Augustsson and Ramanathan included the CO<sub>2</sub> bands in the 10 and 7.6 μ regions in their RCM. They also examined the model's sensitivity to different cloud prescriptions. See **T. Augustsson and V. Ramanathan** 'Radiative-Convective Model Study of CO<sub>2</sub> Climate Problem', *J. Atmos. Sci.* **34** (1977): 448–51. Ramanathan, by this time, had moved to NCAR.



gases.<sup>156</sup> Bolin's new study of the carbon cycle indicated that the depletion of the world's forests could contribute to increasing atmospheric CO<sub>2</sub>.<sup>157</sup> The analyses of the temperature record also suggested that since the mid 1960s the global-scale cooling might have reached a nadir in both hemispheres, although there were still inconsistencies in the data.<sup>158</sup> Moreover, the WMO began to actively support the development of global warming research. At its 28<sup>th</sup> session in 1976, the WMO Executive Committee adopted the first WMO's official statement on climate change, with particular emphasis upon the CO<sub>2</sub> issue. The Executive Committee also decided to organize a scientific workshop on atmospheric CO<sub>2</sub>.<sup>159</sup> The workshop, held in late 1976, made a proposal for the WMO Research and Monitoring Project on Atmospheric Carbon Dioxide, which was approved at the 29<sup>th</sup> session of the Executive Committee in 1977.<sup>160</sup>

As the international meteorological community quickly moved towards global warming research, ERDA created an advisory body, the Study Group on Global Effects of Carbon Dioxide, of which Weinberg was chairman.<sup>161</sup> The Study Group also recommended that ERDA should sponsor a CO<sub>2</sub> research programme. As a first step, the Workshop on Global Effects of Carbon Dioxide from Fossil Fuels was held in March 1977. More than 75 scientists participated in the Workshop, and discussed the current knowledge of the carbon cycle and the potential consequences of increasing atmospheric CO<sub>2</sub>.<sup>162</sup> The Panel on Climate Effects, chaired by Schneider, concluded:

State-of-the-art estimates based on climate models suggest that ... a doubling of CO<sub>2</sub> can cause an increase in global mean temperature of roughly 2 to 3 °C ... Based on

156 **W.C. Wang et al.** 'Greenhouse Effects Due to Man-Made Perturbations of Trace Gases', *Science* **194** (1976): 685-90.

157 **B. Bolin** 'Changes of Land Biota and Their Importance for Carbon Cycle', *Ibid.* **196** (1977): 613-15.

158 **I.I. Borzenkova et al.** 'Variation of Northern Hemisphere Air Temperature from 1881 to 1975', *Soviet Meteorology and Hydrology* **7** (1976): 21-26; **P.E. Damon and S.M. Kunen** 'Global Cooling', *Science* **193** (1976): 447-53; **J.K. Angell and J. Korshover** 'Estimate of Global Change in Temperature, Surface to 100 Mb, Between 1958 and 1975', *Mon. Weather Rev.* **105** (1977): 375-85.

159 **WMO Executive Committee** *Abridged Report with Resolutions - Twenty-Eighth Session of the WMO Executive Committee* (Geneva: WMO, 1976)

160 The invited experts included Broecker, Kellogg, Mitchell, Schneider and others. **WMO Report of the Scientific Workshop on Atmospheric Carbon Dioxide**, WMO series, No. 474 (Geneva: WMO, 1977); **WMO Executive Committee** *Abridged Report with Resolutions - Twenty-Ninth Session of the WMO Executive Committee* (Geneva: WMO, 1977)

161 **D.H. Slade** 'Establishment of an Office of Carbon Dioxide Environmental Effects Research', *Energy Research & Development Administration Announcement* (August 3, 1977): 1-6.

162 The Workshop was divided into four separate panel deliberations. The four panels were; I. Atmospheric CO<sub>2</sub>; II. Biological Effects; III. Ocean Geochemistry; and IV. Climate Effects. **W.P. Elliot and L. Machta** (eds) *Workshop on the Global Effects of Carbon Dioxide from Fossil Fuels*, DOE/CONF-770385 (Washington, DC: US Department of Energy, 1979)

these estimates, significant climate changes could occur by the end of the twentieth century. However, uncertainties in both theory and data render this conclusion tentative; it is quite possible that new information could lower or raise the climate model estimates of warming by a substantial factor.<sup>163</sup>

This had implications for long-term energy planning:

In view of both the potentially large CO<sub>2</sub>-induced climatic effects and the large uncertainties in state-of-the-art estimates of these effects, an important conclusion for energy system planners is the need to maintain energy supply and demand options as open as possible so that, as new information becomes available, shifts in energy strategy would cause minimal disruptions.<sup>164</sup>

The second oil crisis was yet to come, but even so energy was a serious political issue in the US. In April 1977, newly elected President Carter told the nation that the US should wage “the moral equivalent of war” against the looming energy crisis.<sup>165</sup> He sent the National Energy Plan bill to the Congress, proposing to reduce growth in energy demand, reduce oil imports and gasoline consumption, increase coal production, and install insulation and solar energy in homes and businesses. In Oct 1977, a new Department of Energy was established, assuming the responsibilities of FEA, ERDA, and other federal agencies. But CO<sub>2</sub>-induced global warming did not yet become a major political issue. In the analysis of the proposed bill, the Office of Technology Assessment criticized this lack of concern:

Conversion to coal would increase carbon dioxide emissions because, per unit of energy delivered, coal yields 11 percent more carbon dioxide than oil and 67 percent more than natural gas. An accelerated use of coal would therefore aggravate any long-term adverse effects on climate that result from carbon dioxide.<sup>166</sup>

Nevertheless, these concerns about the environmental effects of energy use helped ERDA (later, the US Department of Energy) establish the Office of Carbon Dioxide Effects Research and Assessment Program, which would serve as the main supporting

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163 **S.H. Schneider et al.** 'Panel IV. Climate Effects', in *Workshop on the Global Effects of Carbon Dioxide from Fossil Fuels*, W.P. Elliot and L. Machta (eds) (Washington, DC: US Department of Energy, 1979): 24-33.

164 Ibid.

165 *President Carter's Second Address to the Nation on Energy* (April 18, 1977)

166 **Office of Technology Assessment** *Analysis of the Proposed National Energy Plan* (Washington, DC: US GPO, 1977)

institution for US global warming research.<sup>167</sup>

## How to Approach Global Warming?

### *Modelling versus Observational Studies*

As sketchily illustrated above, the empirical studies of global temperature trend had influenced, albeit not always directly or positively, the way in which the issue of CO<sub>2</sub>-induced global warming was received by both climate scientists and the wider atmospheric sciences community. Callendar's revival of the CO<sub>2</sub> theory of climate change might not have aroused as much interest, had it not been underpinned by the observational studies that were deemed to show a world-wide warming until the mid 20<sup>th</sup> century. The following consensus (that a cooling trend had set in since the 1940s and persisted for the next few decades) might not have diminished a general interest in the global warming hypothesis to any great extent, but did at least convince some scientists that external forcings other than atmospheric CO<sub>2</sub> should play a more important role in controlling climate. When a cooling trend reportedly stalled around the mid 1970s, this encouraged more scientists to look into the climatic effects of increasing CO<sub>2</sub>.

Yet, in the actual development of global warming research during the 1960s and 1970s, the observational studies of past climate and its change occupied a relatively minor place. Geochemical studies of the carbon cycle and physical studies of atmospheric radiation were at the core in the beginning. Not long after, climate sensitivity studies using physico-mathematical models assumed a central role. There had been only a few comprehensive studies of global temperature trend of the past hundred years.<sup>168</sup> Even these studies had limited observation coverage, and the quality of data collection was not uniform. As already noted, the derivation of "global" mean temperatures from sparse and heterogeneous sources was problematic. A number of new empirical analyses that came out in the mid 1970s had a better global coverage and used more reliable observation

<sup>167</sup> **Slade** 'Establishment of an Office of Carbon Dioxide Environmental Effects Research'.

<sup>168</sup> **Callendar** 'Temperature Fluctuations and Trends over the Earth'; **Mitchell** 'Recent Secular Changes of Global Temperature'; **J.M. Mitchell** 'On the Worldwide Scale Pattern of Secular Temperature Change', in *Changes of Climate - Proceedings of the UNESCO/WMO Rome Symposium* (Paris: UNESCO, 1963): 161-81.

sources, but they were confined to the period since the 1950s only.<sup>169</sup> For the temperature trend of the 19<sup>th</sup> and early 20<sup>th</sup> centuries, earlier work (*e.g.*, Mitchell's 1961 paper) was still frequently quoted in the mid to late 1970s.

These shortcomings pointed to the need for more empirical climate studies, not fewer. However, many scientists involved in global warming research, mostly dynamical meteorologists and atmospheric physicists, were rather indifferent to expanding the observational studies of past climate.<sup>170</sup> While these scientists appreciated the painstaking efforts to collect and analyse massive amounts of climatic data, they were critical of an inductive reasoning usually employed by observational climatologists, in which causes and future changes of climate were inferred from the analysis of past climatic records. For them, this actuarial approach was hardly scientific and was of little value in the studies of climate change.<sup>171</sup> It was believed that the problem should be approached deductively. A physico-mathematical basis regarding the detailed inner workings of the climate system would first be postulated, and then the climatic effects of external forcings would be examined by model simulations. Those scientists who conducted both modelling and observational studies of climate change were very rare at the time. Budyko, a Russian climatologist, was probably the only exception.<sup>172</sup>

The modelling approach was, in fact, no less problematic. A standard, hypothetico-deductive testing, even if valid, was not feasible in the studies of future climate changes. There were other complications. In almost all the simulation experiments of CO<sub>2</sub>-induced global warming at the time, every other parameter was held constant, except for a few climate forcings that were being studied, including the atmospheric CO<sub>2</sub> content. In the real world, the parameters governing the state of global climate would change simultaneously. The inherent natural variability of climate would obscure the climate change that could be attributable to the CO<sub>2</sub> increase. Many natural sources of climate variability such as volcanic dust or solar variations would also compete with the added

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169 **Angell and Korshover** 'Estimate of Global Change in Tropospheric Temperature'; 'Estimate of Global Change in Temperature, Surface to 100 Mb, Between 1958 and 1975'; **Damon and Kunen** 'Global Cooling'. However, one study conducted by a Russian group did cover the period since the 19<sup>th</sup> century. See **Borzenkova et al.** 'Variation of Northern Hemisphere Air Temperature from 1881 to 1975'.

170 For instance, the SMIC report, one of the first comprehensive reviews of climate research, mentioned very little about the observational studies of past climate. See **SMIC** *Inadvertent Climate Modification*.

171 **P.M. Kelly** *Interview by the author* (August 4, 1999). For mainstream meteorologists' criticism of observational climatologists, see also Chapter 3.

172 See, *e.g.*, **Budyko** 'Effect of Solar Radiation Variations on Climate of Earth'.

CO<sub>2</sub>. In order to evaluate CO<sub>2</sub>-induced future changes, therefore, the background “noise” level of natural variability should be determined, above which the effects of CO<sub>2</sub> (“signal”) could be identified. As Lamb argued in 1974:

[A]ny possible effects of the increasing scale of human activities upon the global climate can hardly be established until the course of the fluctuations due to natural causes is known, for these change the datum from which the departures due to Man have to be measured.<sup>173</sup>

The significance of observational studies of past climate in global warming research was not entirely ignored by mainstream meteorologists. The 1975 NAS report *Understanding Climatic Change* had a chapter and a long appendix on the study of past climates. The climatic effects of increasing CO<sub>2</sub> were discussed in the section titled “Inference of Future Climates from Past Behavior”, written by an observational climatologist J. Murray Mitchell.<sup>174</sup> As CO<sub>2</sub>-induced global warming emerged as a potential policy issue, not only the model estimates of climatic signals due to the CO<sub>2</sub> effect but also their “detection” came onto the agenda. This required the establishment of the noise level – *i.e.*, the variability of the natural climate. For this reason, at the ERDA Workshop on Global Effects of Carbon Dioxide in 1977, the Panel on Climate Effects recommended a set of research strategies that included the studies of palaeo- as well as recent past climates.<sup>175</sup>

But the main focus of global warming research continued to be placed upon the use and development of climate models, especially GCMs. The value of observational studies of past climates began to be recognized, but slowly and only within the hierarchy of science centred around GCMs. For many scientists, the observational studies of climate and its change were important as long as they could provide a useful source of information for testing the performance of GCMs. For example, the perturbed simulation for some assumed changes in external forcings could be carried out to check the validity of climate models against long-term palaeoclimatic records.<sup>176</sup> In Britain, even this possibility found

173 H.H. Lamb 'Reconstructing the Climatic Patterns of the Historical Past', *Endeavour* **33** (1974): 40-47.

174 USC-GARP *Understanding Climatic Change*.

175 Schneider *et al.* 'Panel IV. Climate Effects'.

176 J. Williams *et al.* 'Simulation of Atmospheric Circulation Using NCAR Global Circulation Model with Ice Age Boundary-Conditions', *J. Appl. Meteorol.* **13** (1974): 305-17; W.L. Gates 'Modeling Ice-Age Climate', *Science* **191** (1976): 1138-44.



little resonance among the Met Office scientists.

### *Simple versus Complex Climate Models*

The focus on GCMs also reflected the hierarchy within the realm of climate modelling. Most of the climate modelling studies in the 1960s to mid 1970s had been performed using simple models – RCMs, other types of radiative transfer models or EBM. One limitation of these models was that they approximated the effects of key physical processes by zonally or globally averaged computations, and did not, or only in a crudely parameterized way, incorporate atmospheric dynamics. They had other deficiencies; as mentioned above, they could not take into account changes of atmospheric variables other than those controlled as parameters of the calculation. To many dynamical meteorologists and atmospheric physicists, this was too restricted, and right from an early stage, it was continually stressed that more complex, 3-D dynamical models of the global atmosphere (*i.e.*, GCMs) should be developed and used to investigate climate change.

Indeed, while discussions at both SCEP and SMIC were by and large based upon simple modelling results, the workshops put a strong emphasis on GCM developments. The CO<sub>2</sub> section of the SCEP report stated that for future global warming research:

We recommend that comprehensive global numerical-dynamical models, including ocean-atmospheric interaction and cloud variation, be developed and applied to the study of expected circulation, precipitation, and temperature patterns for the levels of CO<sub>2</sub> anticipated for the future.<sup>177</sup>

The desirability of using GCMs in global warming research was widely accepted. The problem was that the development and use of GCMs were computationally extremely intensive. Even when only well known dynamical and thermodynamic processes were modelled, excluding many parts of the climate system yet poorly understood, they still required immense computing power and time. For most scientists, these models were simply not an available option. There were simply very few research institutions around the world that could contemplate working on such models.<sup>178</sup> Even for research groups at

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<sup>177</sup> **SCEP** *Man's Impact on the Global Environment*.

<sup>178</sup> In the early 1970s there were only five research institutions that had the capability of developing and deploying GCMs – Geophysical Fluid Dynamics Laboratory (GFDL), National Center for Atmospheric Research (NCAR), NASA Goddard Institute of Space Studies (NASA GISS), and RAND Corporation in the US, and the UK Meteorological Office in Britain. For the development of GCMs in the Met Office, see Chapters 2

these institutions, climate change simulations using GCMs were a very expensive endeavour. Before 1977-8, Manabe's group at GFDL was the only group that employed GCMs in the studies of CO<sub>2</sub>-induced global warming.

Manabe actually began to apply his GCM to the problem of global warming much earlier. He started doing so in around 1970. By the time SMIC was held in 1971, he already had preliminary GCM results for doubled CO<sub>2</sub>.<sup>179</sup> Manabe nonetheless did not formally present these results at the workshop, and instead gave a talk on his new RCM results. The SMIC report did refer to his GCM study but only briefly, and quoted as "private communication".<sup>180</sup> This could have been due to Manabe's cautious personality. And yet, many GCM scientists also thought that, with the computing constraints, it was premature to use GCMs in complex climate change simulations.<sup>181</sup> Other GCM scientists, notably those at the Met Office, were even more cautious. GCMs then had relatively simple parameterization schemes. Manabe's approach was to carry out as many simulation experiments as possible using these rather simple GCMs.<sup>182</sup> The Met Office scientists adopted an almost opposite strategy. They maintained that parameterization schemes should first be improved as much as possible, within computing resources available, before GCMs were to be meaningfully used in such simulations.<sup>183</sup> As noted, the radiative transfer scheme that Manabe willingly used for his GCM simulation of CO<sub>2</sub>-doubling was seen as too rudimentary by the Met Office scientists.

Opinions also differed concerning the question of how valuable simple models could be in the study of global climate change. A number of scientists, for instance Budyko, Sellers, and a Mexican meteorologist Julián Adem, advocated the use of simple climate models not just because access to GCMs was limited, but also because they believed that these models could offer an important insight into some of the most critical physical processes in the climate system.<sup>184</sup> Others were more sceptical. George D. Robinson reviewed

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and 3. For the US development, see **P.N. Edwards** 'A Brief History of Atmospheric General Circulation Modeling', in *General Circulation Model Development - Past, Present, and Future*, D.A. Randall (ed) (San Diego: Academic Press, 2000): 67-90.

179 **S.H. Schneider** *Interview by the author* (January 23, 2001); **Manabe** *Interview by P.N. Edwards*.

180 **SMIC** *Inadvertent Climate Modification*.

181 Manabe recalled that many scientists at the time regarded it as "stupid" to use GCMs in the study of greenhouse warming. See **Smagorinsky** *Interview by S.R. Weart*, **Manabe** *Interview by P.N. Edwards*.

182 **Manabe** *Interview by P.N. Edwards*.

183 **A. Slingo** *Interview by the author* (October 8, 1999)

184 **Budyko** 'Effect of Solar Radiation Variations on Climate of Earth'; **Sellers** 'Reassessment of Effect of CO<sub>2</sub>

simple models for the SCEP workshop and concluded that it might be worthwhile continuing development of such models, but as “educational toys”.<sup>185</sup> The Met Office scientists seemed to agree with this verdict. In describing the modelling work at the Office during the 1970s, Mason, then director-general, later said:

We never bothered with one-dimensional, simple models because we felt that we’re beyond that. In any case, simple one-dimensional models don’t tell you anything about climate because it’s just one column. ... It’s all right for students to play with. But it was not good enough for us when we already had much more advanced models [*i.e.* GCMs] ...<sup>186</sup>

The SMIC report was more accommodating to the value of simple climate models. While the report assigned GCMs a central position within various modelling approaches, it recognized that different models had different but equally valid purposes.<sup>187</sup> Schneider, who himself had been using simple climate models extensively, further developed this middle-way approach. In 1974, in a review paper co-authored with Robert E. Dickinson (a dynamical meteorologist also at NCAR) he introduced the “hierarchy of climate models”, which ranged in complexity from 0- or 1-D EBMs, via 1-D RCMs and 2-D statistical-dynamical models (SDMs), to 3-D GCMs.<sup>188</sup> This model classification system was designed to guide the use and improvement of diverse modelling approaches in climate research. Despite what the term “hierarchy” implied, it could be seen as an attempt to give legitimate status to simple climate modelling. Schneider and Dickinson wrote:

Although it must be recognized that one ultimate objective of mathematical models of climate is to include jointly all the coupled feedback processes in a realistic fashion, it is unlikely that this can be done successfully without the understanding derived from simpler models of individual processes.<sup>189</sup>

This point was made clearer by a quote of Budyko at the end of the paper:

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Variation'; **J. Adem** 'A Critical Appraisal of Simple Climatic Models', in *The Physical Basis of Climate and Climate Modelling* (Geneva: WMO, 1975): 163-70.

185 **G.D. Robinson** 'Review of Climate Models', in *Man's Impact on the Climate*, W.H. Matthews *et al.* (eds) (Cambridge: MIT Press, 1971): 205-15.

186 **B.J. Mason** *Interview by the author* (July 5, 1999)

187 **SMIC** *Inadvertent Climate Modification*.

188 **S.H. Schneider** and **R.E. Dickinson** 'Climate Modeling', *Rev. Geophys.* **12** (1974): 447-93. They also presented the concept of hierarchy of climate models at the GARP study conference on the "Physical Basis of Climate and Climate Modelling" in Stockholm. See **S.H. Schneider** and **R.E. Dickinson** 'Climate Modelling Methodology', in *The Physical Basis of Climate and Climate Modelling* (Geneva: WMO, 1975): 142-47.

Though at present we are far from distinct understanding of the degree of detail in numerical models essential for solving different climatological problems, it is evident that some aspects of climate genesis might be elucidated by means of the simplest existing models, while for studying many questions of climate theory the most general models presently available are insufficient.<sup>190</sup>

However, the concept of hierarchy of climate models, which later became influential in the science of global climate change, did not attract immediate attention. As GCMs matured, simple climate models tended to be marginalized in favour of these complex models.

### ***Different Attitudes towards Policy-relevance***

As the warming effect of increased atmospheric CO<sub>2</sub> was progressively more accepted by those involved in climate research, global warming slowly began to be seen as a policy issue. However, there was a high degree of uncertainty regarding the warming estimate and other climatic consequences. What scientists should do in the face of uncertain climatic effects was understood differently by different scientists or research groups. Such diverse understandings were not unique in the debate over global warming, but could be observed more generally in the field of climate science.

For some scientists like Bryson, as long as there appeared to be significant risks and limited but reasonable scientific underpinnings for them, uncertainty should not be an excuse for complacency or inaction. Bryson was outspoken in his warnings against global climate-food catastrophes. His account of the Sahel drought and monsoon failures, based upon an actuarial reasoning, was rather incomplete. This did not prevent Bryson from forming his controversial view. For him, the role of climate scientists was not so different from that of weather forecasters; they should sometimes be brave enough to inform society of potential climatic hazards and to call for policy action even if the science was uncertain.<sup>191</sup> This perspective was to some extent shared by Lamb in Britain, who did not hesitate to use an actuarial approach to warn of the societal risks from natural climatic

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189 Quoted from **Schneider and Dickinson** 'Climate Modeling', 456.

190 Ibid., 490.

191 **R.A. Bryson** *Interview by the author* (April 27, 2001) Bryson's style of approach was well reflected in his popular science book. See **R.A. Bryson and T.J. Murray** *Climates of Hunger - Mankind and the World's Changing Weather* (Madison: University of Wisconsin Press, 1977)

variations.<sup>192</sup> Similarly relying on contested empirical studies, Lamb also warned that large-scale deliberate climate modification schemes such as the Russian river diversion plan could have serious environmental ramifications.<sup>193</sup>

The actuarial, empirical approach adopted by Bryson and Lamb was considered by many of the mainstream meteorologists to lack a concrete scientific basis.<sup>194</sup> But there were also scientists who were acquainted with the knowledge of atmospheric dynamics and physics, but who at the same time took a precautionary and pro-active position. Schneider was one of the notable examples.<sup>195</sup> He was more modest than Bryson, and was cautious in making bold claims. He firmly believed, though, that scientists should not shy away from discussing the policy implications of the results and limitations of their own research.<sup>196</sup> In 1974, still as a relatively junior scientist, Schneider tried to put the issue of socio-economic impacts of climate change onto the agenda for the GARP study conference on climate modelling, albeit without success.<sup>197</sup> He published numerous popular, semi-scientific articles, highlighting the need for policy discussions about climate change.<sup>198</sup> In his book published in 1976, he himself made a policy proposal – what he called the “Genesis Strategy” to maintain large margins of safety to secure the means of survival, including food reserves, against possible climatic disasters.<sup>199</sup> Moreover, he often included comments that were essentially political in academic journal articles, quite an unusual practice for scientific writing at the time. For example, in 1975, after reviewing various modelling results for doubled CO<sub>2</sub> in *Journal of the Atmospheric Sciences*, he concluded:

The important and perplexing dilemma posed by the present inability of climate theory

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192 **Lamb** *Climate: Present, Past and Future* - Vol. 1. See also Chapter 3.

193 **H.H. Lamb** 'Reply to "The Russian Project"', *Weather* **25** (1970): 472-73; 'Climate Engineering Schemes to Meet a Climatic Emergency', *Earth-Sci. Rev.* **7** (1971): 87-95. See also Chapter 3.

194 Interestingly, both Bryson and Lamb were sceptical of the CO<sub>2</sub>-induced global warming hypothesis.

195 Revelle, Bolin and Kellogg, among others, were also active in promoting policy discussions concerning climate change. But they were perhaps more in tune with the mainstream scientific community.

196 **Schneider** *Interview by the author*.

197 *Ibid.*

198 **S.H. Schneider** 'The Population Explosion - Can it Shake the Climate?' *Ambio* **3** (1974): 150-55; 'Specter at the Feast', *The National Observer* (July 6, 1974): 18; 'Public-Policy Issues in Climatic Change', *EOS T. Am. Geophys. Un.* **57** (1976): 893-94; **W.W. Kellogg and S.H. Schneider** 'Climate Stabilization - for Better or for Worse', *Science* **186** (1974): 1163-72; **Schneider and Dennett** 'Climatic Barriers to Long-term Energy Growth'; **S.H. Schneider and L.E. Mesirow** 'Genesis Strategy - Climate and Global Survival', *Futurist* **10** (1976): 192-97.

199 Schneider took the term "Genesis" from a biblical story of ancient climate forecasting, in which Joseph advised the Pharaoh of Egypt to store part of the abundance for the seven years of feast so that he could feed



and modeling to offer much more than an order-of-magnitude estimate of the climatic effects of increased CO<sub>2</sub> is that the seriousness of potential climatic risks of continued use (or social risks of abandoned use) of fossil fuel to the year 2000 and beyond range from negligible to extreme ... Since the consequences of a climate change at the higher end of the current estimate could be both enormous and possibly irreversible, perhaps society would be best to err conservatively in planning future fossil fuel consumption patterns – and in any case should consider what preparations need to be made to adjust to such a dramatic change ...<sup>200</sup>

Schneider's willingness to talk publicly about the policy implications of his research often upset mainstream meteorologists and atmospheric scientists.<sup>201</sup> For them, large uncertainties meant that more research was required before any policy discussions could take place. Until then, they believed, scientists should focus on science, nothing else. A pro-active stance such as Schneider's was not something to be commended for responsible scientists. This mode of thinking was, in a sense, no less political because it presupposed a particular model of what a prudent policy should be like. Most scientists nevertheless saw it as unproblematically separated from politics.

It is interesting that Mason and his staff at the Met Office often held this view more rigidly. The Met Office was a government service agency, and the Office scientists were accustomed to mission-oriented research aimed at policy relevant results. As far as the issue of climate change was concerned, however, they thought that the scientific basis was too weak. The policy discussions put forward by some climate scientists were seen as "too political" or "emotional".<sup>202</sup> In the RMS discussion in 1970, Mason acknowledged the societal importance of climate change, but emphasized that scientists should "do well as a profession to replace much of the present emotion by some hard facts and figures".<sup>203</sup> When he revisited the topic in 1976, he again argued that there was no need for panic. What was needed first was to investigate global climate with bigger and better models.<sup>204</sup>

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the people during the seven years of famine. See **Schneider and Mesirow** *The Genesis Strategy* .

200 **Schneider** 'On the Carbon Dioxide-Climate Confusion' .

201 For this reason, he was practically fired from NASA GISS by Robert Jastrow, then director, who did not much appreciate the study of anthropogenic climate change. Schneider was "saved" by W.W. Kellogg at NCAR.

**Schneider** *Interview by the author* . Interestingly, Jastrow later became one of the vocal global warming sceptics in the 1990s.

202 **Mason** *Interview by the author* .

203 **Lamb** 'Climatic Variation and Our Environment Today' .

204 **J. Gribbin** 'Man's Influence Not Yet Felt by Climate', *Nature* **264** (1976): 608.

Even when in 1972-75 the Met Office was involved in the Committee on Meteorological Effects of Stratospheric Aircraft (COMESA), a more explicitly policy-relevant research project, the Office scientists confined themselves to technical discussions.<sup>205</sup> In Mason's words, the Met Office's position was: "We'll do the science. Let science speak for itself."<sup>206</sup>

These differences were in part moulded by personal backgrounds. Bryson was a wartime forecaster, and that experience had a strong effect on his way of doing science.<sup>207</sup> Lamb's style of inferring from scant observations was also related to his forecasting experience during World War II.<sup>208</sup> Their efforts to put human affairs at the centre of climatology were motivated by their long interest in geography, archaeology and human history.<sup>209</sup> Schneider's activism came from an entirely different direction. Originally trained as a mechanical engineer majoring in plasma physics, he was influenced by the wave of environmental awareness during the late 1960s and early 1970s.<sup>210</sup> Around the time of the first Earth day in 1970, he attended the lecture of Barry Commoner, a scientist and environmental activist, on human impact on climate. Captivated by Commoner's lecture, Schneider took a summer job as a computer programmer for space scientist Rasool, who asked him to model the climatic effects of aerosol and CO<sub>2</sub>.<sup>211</sup> The 1971 Rasool-Schneider paper was the outcome of this.

Institutional settings were also important. Lamb left the Met Office in the early 1970s to set up the Climatic Research Unit (CRU) at the University of East Anglia, while Bryson had already been running his own university climate research group since the early 1960s. By contrast, the Met Office scientists could not enjoy the freedom that Bryson and Lamb

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205 See Chapter 3 for more about COMESA.

206 **Mason** *Interview by the author*.

207 **R.A. Bryson** 'Typhoon Forecasting, 1944, or, The Making of a Cynic', *B. Am. Meteorol. Soc.* **81** (2000): 2393-97.

208 As noted in Chapter 3, when World War II broke out, Lamb was assigned to work on the meteorology of gas spraying. But he was a Quaker and resigned from the Met Office. He was later reinstated as an aviation forecaster at the Irish Meteorological Service. During his days at the Irish Meteorological Service, Lamb had to draw weather charts with very little data over the North Atlantic and Southern Oceans. Because Ireland was a neutral country, the British Meteorological Office was not sending classified meteorological data to the Irish Meteorological Service. This experience may have helped Lamb embark on his work on the reconstruction of past weather maps. **H.H. Lamb** *Through All the Changing Scenes of Life - A Meteorologist's Tale* (East Harling: Taverner, 1997)

209 *Ibid.*; **Bryson** *Interview by the author*.

210 His choice of plasma physics was also to do with his environmental concern. He thought that it could help develop renewable energy. **Schneider** *Interview by the author*.

211 *Ibid.*; **J. Kaiser** 'Taking a Stand: Citizen-Scientist Guru', *Science* **287** (2000): 1189.

had. The Met Office was a fairly hierarchical organization. The scientific officers were supposed to concentrate on their assigned research jobs, and the policy matters were strictly left to a few senior staff. This compartmentalization was further reinforced by the prevailing old-fashioned attitude, “I’m a scientist, and therefore I’ve nothing to do with politics”.<sup>212</sup> NCAR, where Schneider was a member of its climate group headed by Kellogg, had a very different atmosphere. Funded by NSF, it was less coordinated and less hierarchical. Individual scientists had relative autonomy over their research. Thus, although Francis Bretherton, director of NCAR since 1974, and a number of dynamical meteorologists inside were critical of the type of policy-oriented research work that Kellogg’s climate group was producing, this did not place serious constraints on Schneider’s activities.<sup>213</sup>

Later, as public concern over global warming grew, climate scientists became more accepting of and open to policy discussions. But these differences in attitudes, and the tension between them, continued to exist and served as a source of controversy.

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212 **Slingo** *Interview by the author*.

213 **Schneider** *Interview by the author*.

## Chapter 5. Establishing the Science of Global Warming

### Beginning of Global Warming Research in Britain

#### *The Meteorological Office*

Despite the cautious attitude adopted, the Met Office initiated its own global warming research programme in around 1977. The growth of global warming research in the US provided a strong stimulus to the Met Office scientists. Given the hierarchical culture of the Office, however, individual scientific officers could not instigate global warming research at their own discretion. The decision had to be made at a higher level. It was John Mason, the director, who took the initiative. Mason always wanted to build the Met Office into one of the world's leading meteorological institutions, and saw advances in numerical modelling as a crucial part of this.<sup>1</sup> Since assuming directorship in 1965, he had thus been an enthusiastic supporter of the Met.O.20's general circulation model (GCM) work. Later in the mid 1970s, he became more personally interested in the modelling studies of climate, writing and giving lectures himself about the topic.<sup>2</sup> Through these, he recognized that the issue of anthropogenic climate change was rapidly becoming an important, international agenda. By 1976-77, with the mounting US research activities in CO<sub>2</sub>-induced global warming, especially the GCM studies by Syukuro Manabe and his co-workers at the Geophysical Fluids Dynamics Laboratory (GFDL), he felt that the Met Office should not fall behind in that area. Although he was still dubious about the risk of global warming, Mason encouraged Andrew Gilchrist, who had been head of the Met.O.20 and was promoted to deputy director of dynamical research in 1977, and other Met.O.20 scientists to follow suit.<sup>3</sup>

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<sup>1</sup> **B.J. Mason** 'Some Thoughts on the Future', *Weather* **20** (1965): 273-74; 'Meteorological Office - Today and Tomorrow', *Contemp. Phys.* **8** (1967): 63-86; 'Future Developments in Meteorology - An Outlook to Year 2000', *Q. J. Roy. Meteor. Soc.* **96** (1970): 349-68.

<sup>2</sup> **B.J. Mason** 'Towards the Understanding and Prediction of Climatic Variations', *Q. J. Roy. Meteor. Soc.* **102** (1976): 473-98; 'Nature and Prediction of Climatic Changes', *Endeavour* **35** (1976): 51-57; **J. Gribbin** 'Man's Influence Not Yet Felt by Climate', *Nature* **264** (1976): 608..

<sup>3</sup> **A. Gilchrist** *Interview by the author* (June 9, 1999) However, Folland suggests that it was Gilchrist, rather than Mason, who pushed the Met.O.20 towards global warming research. **C.K. Folland** *Interview by the author* (October 5, 1999)

The increasing activities of the World Meteorological Organization (WMO) in this area also had some impact on the course of the Met Office's climate research. The Met Office, as the national meteorological agency, was automatically a member organization of WMO, and was bound to be responsive to its research initiatives. As described in Chapter 4, WMO held a scientific workshop on global warming in 1976, and approved the launch of a Research and Monitoring Project on atmospheric CO<sub>2</sub> in 1977. An expert group on CO<sub>2</sub> was set up to advise the WMO Executive Committee on the implementation of the project. In the same year, WMO issued a technical report on anthropogenic climate change, following the request addressed by this group in the Executive Committee's 28<sup>th</sup> session. Written by William W. Kellogg, the report stated that CO<sub>2</sub> would be the single most important factor affecting global climate change.<sup>4</sup> In early 1978, WMO co-sponsored the Workshop on Carbon Dioxide, Climate and Society, together with the International Institute for Applied Systems Analysis (IIASA), the United Nations Environment Programme (UNEP), and the Scientific Committee on Problems of Environment (SCOPE) of the International Council of Scientific Unions (ICSU).<sup>5</sup> These consecutive events pushed the Met Office scientists to take the issue of global warming more seriously.

There were other circumstances that gave the Met Office an incentive to enter into the studies of global warming at that time. The summers of 1975-76 were exceptionally hot and dry in Western Europe. In Britain, the drought during this period was reported to mark the severest in more than two centuries of records, producing "a sea-change in the British attitude to water supplies and the consequences of water shortages".<sup>6</sup> The occurrence of such weather anomalies was not taken to indicate global warming,<sup>7</sup> but increased the British government's interest in climate change. Partly in response to this, a small interdepartmental liaison group on climatology was organized, in which Mason took an instrumental part.<sup>8</sup> The group soon started to look into the likely effects of various possible climate changes, supporting the Met Office's move towards global warming

4 **W.W. Kellogg** *Effects of Human Activities on Global Climate*, WMO Technical Note, No. 156 (Geneva: WMO, 1977)

5 **J. Williams** (ed.) *Carbon Dioxide, Climate and Society - Proceedings of a IIASA Workshop* (Oxford: Pergamon Press, 1978)

6 **W.J. Burroughs** *Does the Weather Really Matter? - The Social Implications of Climate Change* (Cambridge: Cambridge University Press, 1997)

7 R.A.S. Ratcliffe, a climatologist at the Met Office, argued: "There appears to be no evidence to suggest a climatic change or trend; rather the drought stands out as an isolated but very unusual event." **R.A.S. Ratcliffe** 'Meteorological Aspects of 1975-76 Drought', *P. Roy. Soc. Lond. A. Mat.* **363** (1978): 3-20.



research. Nuclear optimism, which tended to divert attention from the climatic effects of fossil fuel energy production, was also fading away. The government's plan to build the new thermal oxide reprocessing plant (THORP) in Windscale, and an ensuing radioactive leak incident, caused doubts about the safety of nuclear energy. After the Windscale public inquiry in 1977, public opposition to nuclear energy grew even stronger.<sup>9</sup> This further raised concerns about energy futures, though not as prominently as in the US, and could potentially help open the way for more discussions about CO<sub>2</sub>-induced global warming.

The Met Office's first CO<sub>2</sub>-doubling experiment was performed by P. Graystone at the Met.O.20 in early 1977. He used a single column of the Met.O.20 GCM, as a 1-D radiative-convective model (RCM), by eliminating the dynamics and leaving the parameterizations of physical processes (*e.g.*, radiation, convection, precipitation and cloudiness, and boundary-layer).<sup>10</sup> Some months later, a more complete version of the same experiment was conducted by Peter Rowntree and Julia Walker (later, Julia Slingo), who were developing an 11-level GCM for the Met.O.20's tropical meteorology group.<sup>11</sup> The results were presented to the IIASA Workshop in 1978.<sup>12</sup> While the mean temperature increases of 1.5–2.5 °C for doubled CO<sub>2</sub> were generally consistent with those of Augustsson and Ramanathan, the detailed results turned out to be quite sensitive to the assumptions made for the temperature and humidity structure (*i.e.*, water vapour and cloud feedback); subtle changes in the treatment of moisture in a convective parameterization scheme gave the greater effect of doubling CO<sub>2</sub> for surface temperatures above 280 K, but made little difference at lower temperatures.<sup>13</sup> Yet, as Julia Slingo later suggested, far more important in this work was the development of a realistic convection parameterization scheme for the Met.O.20 11-level GCM.<sup>14</sup> After this experiment, RCM studies of the greenhouse effect were no longer carried out at the Met.O.20.

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8 **B.J. Mason** *Interview by the author* (July 5, 1999)

9 **W. Rudig** 'Maintaining a Low Profile - The Anti-Nuclear Movement and the British State', in *States and Anti-Nuclear Movements*, H. Flam (ed.) (Edinburgh: Edinburgh University Press, 1994): 70-100.

10 **P. Graystone** *A Note on the Effect of Changes in CO<sub>2</sub> Concentration on Climate using Numerical Models*, Met.O.20 Technical Note, No. II/115 (Bracknell: British Met Office, 1977)

11 **P.R. Rowntree and J. Walker** *The Effects of Doubling the CO<sub>2</sub> Concentration on Radiative-Convective Equilibrium*, Met.O.20 Technical Note, No. II/119 (Bracknell: British Met Office, 1978)

12 **P.R. Rowntree and J. Walker** 'The Effects of Doubling the CO<sub>2</sub> Concentration on Radiative-Convective Equilibrium', in *Carbon Dioxide, Climate and Society - Proceedings of a IIASA Workshop*, J. Williams (ed.) (Oxford: Pergamon Press, 1978): 181-91.

13 Later, R.S. Lindzen at MIT put forward a similar argument, and used it against the CO<sub>2</sub>-induced global warming hypothesis. See below.

14 **J.M. Slingo** *Interview by the author* (October 7, 1999)

The fuller investigations began in earnest in late 1977 when John F.B. Mitchell was assigned to work on GCM studies of the greenhouse effect. In 1978, the Met.O.20 created the climate modelling sub-branch, headed by Rowntree, lending additional support to John Mitchell's work.<sup>15</sup> Again, the Met.O.20 took a distinctly conservative GCM approach. The treatment of the ocean was a case in point. The ocean plays a key role in climate change, transporting heat spatially and controlling the seasonal temperature variation by virtue of its large thermal capacity. Dynamical ocean modelling was in its infancy at the time, and the common fix was to treat the ocean as wet surface, as Manabe and Wetherald did in their 1975 study.<sup>16</sup> This "swamp" method had an advantage in the perturbed simulations, since the exclusion of the ocean's thermal capacity would only delay the attainment of equilibrium between the control and perturbed (e.g., CO<sub>2</sub>-doubling) runs. The weakness was that the simulation of the current climate would be deficient, since factors such as the seasonal cycle and the transport of heat in the ocean were not taken into account. For the Met.O.20 scientists, the assessment of the climatic effects of increased CO<sub>2</sub> should not precede the realistic simulation of the present climate. Their 5-level GCM already had a finer horizontal resolution and more sophisticated parameterization schemes than Manabe's model.<sup>17</sup> By 1976-77, a simple model of the upper layers of the ocean had also been tentatively developed.<sup>18</sup> But these were not enough for the Met.O.20 scientists. They preferred to include the seasonally and regionally varying effects of oceanic heat by prescribing the sea surface temperatures and

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15 **J.F.B. Mitchell** *Interview by the author* (October 8, 1999); **P.R. Rowntree** *Interview by the author* (October 4, 1999); **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1978). For the background in which a new climate modelling group began, see Mason's future plans for the Met Office. **B.J. Mason** 'The Future Development of the Meteorological Office', *Meteorol. Mag.* **107** (1978): 129-40. He wrote: "Concern with the environment and the possible effects of man-made activities on weather and climate is likely to persist and to become of increasing political and economic importance. The Office should therefore give increased attention to understanding the physical basis of climate and to developing models to simulate, and perhaps eventually to predict, climate changes both natural and man-made. ... This will require some considerable expansion of the Dynamical Climatology Branch."

16 **S. Manabe and R.T. Wetherald** 'Effects of Doubling CO<sub>2</sub> Concentration on Climate of a General Circulation Model', *J. Atmos. Sci.* **32** (1975): 3-15.

17 The Met.O.20 5-level GCM had a quasi-uniform resolution of 3° x 5° (latitude/longitude). The model grid was approximately square of ca. 330 km in middle latitudes. On the other hand, Manabe's GCM had a horizontal resolution of 4.5° x 5° (latitude/longitude), which was equivalent to the grid size of ca. 500 km in middle latitudes. See **G.A. Corby et al.** 'United Kingdom Meteorological Office Five-Level General Circulation Model', in *General Circulation of the Atmosphere*, J. Chang (ed.), *Methods in Computational Physics*, Vol. 17 (New York: Academic Press, 1977): 67-110; **A. Gilchrist** 'The Meteorological Office 5-Layer General Circulation Model', in *Report of the JOC Study Conference on Climate Models*, W.L. Gates (ed.) (Geneva: WMO/ICSU, 1979): 254-95; **S. Manabe et al.** 'Global Ocean-Atmosphere Climate Model - 1. Atmospheric Circulation', *J. Phys. Oceanogr.* **5** (1975): 3-29.

18 **J.F.B. Mitchell** *One Dimensional Modelling of the Upper Layers of the Ocean*, Met.O.20 Technical Note, No. II/61 (Bracknell: British Met Office, 1976); *Some Experiments using a Simple Oceanic Mixed Layer Model*, Met.O.20 Technical Note, No. II/86 (Bracknell: British Met Office, 1977)

the sea-ice distribution. The resultant infinite thermal capacity of the ocean meant that they could only have the responses from the atmosphere and land surface. Without a proper ocean model, however, it was considered a better option.<sup>19</sup>

The Met Office's tendency to emphasize the simulation of the presently observed climate partially stemmed from its early interest in long-range dynamical weather forecasting.<sup>20</sup> Modelling climate and its change was of course a different task from long-range forecasting in many respects. This tendency nonetheless became built into the general modelling philosophy and continued to have influence on the Met.O.20's GCM practices. The meteorologists' view was also reflected in the fact that the Met.O.20 scientists were interested from the beginning in regional as well as seasonal climatic variations due to increased CO<sub>2</sub>. Many other scientists involved in global warming research elsewhere were then preoccupied with the global annual mean temperature increase. Thus, whereas the scientists were tackling a seemingly "global" issue, their approaches were very much locally grounded.

With the specified sea surface temperatures, the increase in atmospheric CO<sub>2</sub> could give only a small model response. The preliminary results by John Mitchell showed that in the winter the model response to doubled CO<sub>2</sub> was difficult to separate from the natural variation expected between two model runs. In the summer hemisphere, a mean increase of surface temperature was less than 1 °C, with a 5 percent reduction in rainfall over the oceans. The annual mean temperature increase was only about 0.2 °C. Even when CO<sub>2</sub> was increased tenfold, the estimate did not exceed 1 °C, which was quite unrealistic.<sup>21</sup> To solve the problem, the sea surface temperatures were assumed to increase by 2 °C everywhere for doubled CO<sub>2</sub>, the magnitude of the increase being chosen on the basis of simple climate modelling experiments, and a further integration was performed. This approximation was rather arbitrary and contestable, but the Met.O.20 scientists believed that it would provide a "reasonable" indication of regional and seasonal variations to be expected in the response to increased CO<sub>2</sub>. The calculation led to an average surface warming of 2.7 °C, close to the temperature increase obtained by Manabe and

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<sup>19</sup> **Mitchell** *Interview by the author*.

<sup>20</sup> See Chapter 2.

<sup>21</sup> **J.F.B. Mitchell** *Preliminary Report on the Numerical Study of the Effect on Climate of Increasing Atmospheric Carbon Dioxide*, Met.O.20 Technical Note, No. II/137 (Bracknell: British Met Office, 1979)

Wetherald.<sup>22</sup>

The Met Office's global warming research was largely dominated by the modelling studies, though the Met.O.13 also made a modest contribution. For example, during 1977-78, M.K. Miles and P.B. Gildersleeves statistically examined observed temperature changes in the Northern hemisphere over the last century, and the extent to which these changes could be correlated with changes in atmospheric CO<sub>2</sub> and volcanic dust.<sup>23</sup> Perhaps more importantly, in 1979, P.G.F. Caton commenced the creation of a fully quality-controlled set of monthly mean sea surface temperatures, from which the variability of mean temperature and its trends could be investigated more closely.<sup>24</sup> This data set was not initially intended to be used for the studies of global warming, but later became an important element in "detection" studies.

### *Climatic Research Unit*

Outside the Met Office, the indifference towards CO<sub>2</sub>-induced global warming was still prevalent in Britain. There were some modelling studies that had relevance to the question of climate. The UK Universities' Atmospheric Modelling Group, a consortium of university groups centred at Reading, was making progress in building its own GCM, funded by the Natural Environment Research Council (NERC).<sup>25</sup> At Imperial, there were some attempts to animate John Green's 2-D statistical-dynamical model (SDM) approach.<sup>26</sup> Atmospheric and oceanic models of various scales and complexities were also being experimented with by applied mathematics groups at Exeter and Cambridge respectively.<sup>27</sup> To a great extent, however, most of these studies were theoretically oriented, and the issues like global warming had never been on the agenda.

22 **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1979)

23 **M.K. Miles and P.B. Gildersleeves** 'Statistical Study of Likely Causative Factors in Climatic Fluctuations of Last 100 Years', *Meteorol. Mag.* **106** (1977): 314-22; 'Statistical Study of Likely Influence of Some Causative Factors on Temperature Changes since 1665', *Meteorol. Mag.* **107** (1978): 193-204.

24 **British Met Office** *Annual Report, 1979*; **P.G.F. Caton and H.D. Lawes** *Analysis of Historic Sea Surface Temperature Data*, Met.O.13 Memorandum No. 103 (Bracknell: British Met Office, 1980)

25 **R.P. Pearce and B.J. Hoskins** 'Advances in Atmospheric Circulation Studies', *NERC News* **1** (1987): 5-9; **B.J. Hoskins and A.J. Simmons** 'Multilayer Spectral Model and Semi-Implicit Method', *Q. J. Roy. Meteor. Soc.* **101** (1975): 637-55.

26 **A.A. White** 'Surface Flow in a Statistical Climate Model - Test of a Parameterization of Large-scale Momentum Fluxes', *Q. J. Roy. Meteor. Soc.* **103** (1977): 93-119.

27 See, for example, **H.B. Gordon and D.R. Davies** 'Mathematical Prediction of Climatic Change', *Nature* **253** (1975): 419-20; **P.J. Everson and J.C. Perfect** 'Determination of Predictability of a 4-Level Geostrophic Model Using Effects of Grid Point Numerical Errors', *Tellus* **29** (1977): 588-92; **J.E. Hart and P.D. Killworth** 'Open Ocean Baroclinic Instability in Arctic', *Deep-Sea Res.* **23** (1976): 637-45; **P.D. Killworth** 'Mixing on Weddell Sea Continental Slope', *Deep-Sea Res.* **24** (1977): 427-48.

Oxford's atmospheric physics group had earlier collaborated with the Met.O.20 on GCM radiative transfer parameterizations, and could have pursued RCM studies of global warming. Led by John Houghton, the group turned instead to remote sounding measurements from satellites since the early 1970s. Its simple modelling studies were restricted to the stratosphere.<sup>28</sup> Close to the simple climate modelling experiments of Budyko-Sellers type was the work of astrophysicists at Leicester, who studied the role of surface albedo and atmospheric composition as factors affecting the earth's radiation balance. But the Leicester group was interested in these processes because of their relevance to the formation of planetary atmospheres. The greenhouse effect was discussed only in the context of the evolution of the earth's atmosphere, rather than of human impact on climate.<sup>29</sup>

Neither was the topic of global climate change, whether natural or anthropogenic, popular among empirically oriented climatologists. In 1976, the NERC reviewed the status of climate research in British universities.<sup>30</sup> The majority of research groups were based in geography departments. They were mainly involved in "applied climatology", focusing upon more immediate societal and regional applications, as opposed to "world climatology". In the field of the empirical study of current world climate, there were no more than 19 research workers. Only four out of them were devoting a major part of their time to the research; for the rest, it was just a part-time interest.<sup>31</sup> The situation was no better for the study of climate changes over the last hundreds of years. Hubert Lamb's Climatic Research Unit (CRU) at East Anglia was the only British institution delving into this line of research. Palaeoclimatological studies using proxy data were being done by a number of geology and palaeobotany research groups, but their findings were rarely discussed in connection with the issue of present and future climate changes.<sup>32</sup>

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28 **J.T. Houghton and S.D. Smith** 'Remote Sounding of Atmospheric Temperature from Satellites - 1. Introduction', *Proc. R. Soc. Lon. Ser.-A* **320** (1970): 23-32; **R.S. Harwood and J.A. Pyle** '2-Dimensional Mean Circulation Model for Atmosphere Below 80km', *Q. J. Roy. Meteor. Soc.* **101** (1975): 723-47.

29 See, e.g., **A. Henderson-Sellers and A.J. Meadows** 'Surface Temperature of Early Earth', *Nature* **270** (1977): 589-91; **A. Henderson-Sellers** 'Earth's Evolution and Paleoclimatology - Computational Model', *Comput. Geosci.* **4** (1978): 319-31; **A. Henderson-Sellers and A.J. Meadows** 'Simplified Model for Deriving Planetary Surface Temperatures as a Function of Atmospheric Chemical Composition', *Planet. Space Sci.* **27** (1979): 1095-99.

30 **NERC Reports of the Working Parties on Applied Climatology and on World Climatology** (London: NERC, 1976)

31 *Ibid.*

32 See, e.g., **N.J. Shackleton** 'Oxygen Isotope Stratigraphic Record of Late Pleistocene', *Philos. T. Roy. Soc. B* **280** (1977): 169-82.



The issue of global warming was not unnoticed by British academic meteorologists and climatologists. Through personal contacts and international scientific journals, they were aware of the development of global warming research in the US and the Met Office. Reviews of anthropogenic climate change, including CO<sub>2</sub>-induced global warming, also appeared from time to time in British journals and publications.<sup>33</sup> This awareness nevertheless did not lead British university researchers to undertake new projects on the climatic effects of increased CO<sub>2</sub>. They were quite content and absorbed in what they had been doing. Furthermore, many dynamical meteorologists – and some climatologists as well<sup>34</sup> – adopted the climate modellers' view that the use of GCMs would ultimately be necessary to investigate global climate and its change.<sup>35</sup> Even if they wanted to embark on global warming research, their limited access to computing power and time would have been a serious obstacle.

In fact, the empirical studies of past climates especially during historical times, could be readily linked to global warming research. Observational climatologists were the first to underline this point. For instance, as has been noted, Lamb had argued early on that the observational studies were essential if the actual human impact on climate were to be identified and thoroughly investigated. This does not mean that he was attracted to the global warming hypothesis. On the contrary, he thought that natural climate variations and changes were more important than the global effects of anthropogenic CO<sub>2</sub>. For he believed that the climate system had the capacity to respond and adapt to relatively small changes in external forcings.<sup>36</sup> Lamb was also critical of the reliability of the numerical

33 See **R.G. Barry** 'Short-term Climatic Fluctuations', *Prog. Phys. Geog.* **1** (1977): 114-25; **J.G. Lockwood** *Causes of Climate* (London: E. Arnold, 1979); **J. Gribbin** (ed.) *Climatic Change* (Cambridge: Cambridge University Press, 1978). For popular science books, see **J. Gribbin** *Forecasts, Famines and Freezes - Climate and Man's Future* (London: Wildwood House, 1976); *The Climatic Threat: What's Wrong with Our Weather?* (London: Fontana, 1978); **M.W. Holdgate** *A Perspective of Environmental Pollution* (Cambridge: Cambridge University Press, 1979); **C. Tickell** *Climatic Change and World Affairs* (Oxford: Pergamon Press, 1977). John Houghton also briefly reviewed the greenhouse effect in early 1979 although he did not pursue further study. **J.T. Houghton** 'Greenhouse Effects of Some Atmospheric Constituents', *Philos. T. Roy. Soc. A* **290** (1979): 515-21.

34 See, e.g., **B.W. Atkinson** 'Atmosphere - Recent Observational and Conceptual Advances', *Geography* **63** (1978): 283-300.

35 Green was one of the very few dynamical meteorologists who were openly critical of the focus on GCMs. As briefly noted in Chapter 3, he complained in 1978 that the Met Office "spends about as much as on running their [GCMs] as goes into university education of meteorologists in Britain". **G.J. Shutts and J.S.A. Green** 'Mechanisms and Models of Climatic Change', *Nature* **276** (1978): 339-42.

36 **P.M. Kelly** *Interview by the author* (August 4, 1999). Until he died, Lamb continued to hold this view. In one of his interviews, conducted in 1994, he said: "Because of man's impressive scientific achievements, ... there is also the tendency to think that Man is responsible for all occurrences. That should be viewed with considerable scepticism." See **H. Taba** 'The Bulletin Interviews - H. H. Lamb', *WMO Bulletin* **43** (1994): 99-110.

modelling approach, upon which most global warming studies were dependent.<sup>37</sup> His sceptical view was well reflected in his second volume of *Climate: Present, Past and Future*, published in 1977. He did mention the CO<sub>2</sub> problem in this book, but only briefly. Out of 20 chapters, over 800 pages, one chapter of 23 pages was devoted to the topic of anthropogenic climate change. The CO<sub>2</sub> section was only two pages long.<sup>38</sup> Although the only modelling study he cited was the 1967 RCM results of Manabe and Wetherald, and the modelling studies since then did not, on the whole, point to a lesser warming, he concluded:

On balance, the effect of increased carbon dioxide is almost certainly in the direction of warming but is probably much smaller than the estimates which have commonly been accepted.<sup>39</sup>

Lamb's scepticism about CO<sub>2</sub>-induced global warming did not prevent CRU from getting financial support for related purposes. In 1976, the US Environmental Protection Agency (EPA) became interested in the issue of global warming. J. Murray Mitchell of the National Oceanic and Atmospheric Administration (NOAA)<sup>40</sup> knew that Lamb and his co-workers were working on the studies of past climates, and channelled the EPA funding to the Unit.<sup>41</sup> A year later, the US Department of Energy (USDOE) emerged as the major agency supporting global warming research and took over the EPA's funding for CRU. This US funding enabled Lamb to employ a few more research staff, expanding ongoing research on the reconstruction of past climates over the last hundreds of years, and launching a new research programme in dendroclimatology.<sup>42</sup> Around the same time, P. Mick Kelly at the Unit was able to secure extra funding from USDOE. The lack of homogeneous temperature data for the 19<sup>th</sup> and 20<sup>th</sup> centuries was what made it difficult to detect the supposed warming effects of increased CO<sub>2</sub> and to validate the long-term model integration. Capitalizing on this, Kelly, together with Raymond Bradley at the University

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37 **Kelly** *Interview by the author*. This critical stance might have been strengthened by his personal animosity towards the Met Office. He felt that he had been treated quite poorly by the Met Office senior scientists, who strongly advocated physico-mathematical modelling.

38 **H.H. Lamb** *Climate: Present, Past and Future - Vol. 2 Climatic History and the Future* (London: Methuen, 1977)

39 *Ibid.*

40 In 1965, the US Weather Bureau became a component of the newly formed Environmental Science Services Administration (ESSA). Later in 1970, ESSA became the National Oceanic and Atmospheric Administration (NOAA).

41 **T.M.L. Wigley** *Interview by the author* (August 14, 2000)

42 *Ibid.*; **P.D. Jones** *Interview by the author* (August 15, 2000)

of Massachusetts-Amherst and Henry Diaz at NOAA, submitted a joint research proposal to USDOE, and were contracted to develop a global database of temperature data, based on more reliable, quality-controlled sources.<sup>43</sup>

By this stage, scientists at CRU saw global warming as more of a useful source of funding for their work. Their observational studies of global climate were not as yet organized in direct response to the question of whether the increase in atmospheric CO<sub>2</sub> was already having an impact. The Unit was also conducting a range of different, more regionally focused research projects. However, the shift in its research activities was beginning to take place.

The transition was gradual, but was highlighted by Lamb's retirement as director in 1978. A new director was Tom Wigley, who joined CRU in 1975 as leader of the historical climatology project funded by the Rockefeller Foundation. Lamb would have liked the Unit to continue its early efforts to collect and interpret climatic records and to reconstruct past weather maps in a qualitative fashion.<sup>44</sup> Originally trained as a physicist, Wigley was critical of Lamb's approach. He strongly emphasized the importance of data verification, and urged the application of rigorous statistical techniques in handling climatic data, pushing the Unit in a more quantitative direction.<sup>45</sup> Unlike his predecessor, Wigley was more receptive both to the power of climate modelling and to the importance of global warming research. Given his background, this could be expected. Upon completing his PhD in mathematical physics in Australia, he had worked as a professor of mechanical engineering for several years in Canada. The research themes that he had been involved with included numerical analysis of the dynamics and dispersion of the industrial plumes and carbon isotope geochemistry. Even after joining CRU, Wigley occasionally published on these topics.<sup>46</sup> His knowledge of atmospheric pollution and CO<sub>2</sub> geochemistry,

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43 But the actual funding was delayed for a few years. **Kelly** *Interview by the author*, **T.S. Feldman** *Online Electronic Mail Interview with Raymond S. Bradley*, <http://www.agu.org/history/sv/temperature/bradleyinterview.html> (American Geophysical Union Geophysics History Project: September 30, 2000)

44 See Chapter 3.

45 **Wigley** *Interview by the author*; **Jones** *Interview by the author*. See also **M.J. Ingram et al.** 'Historical Climatology', *Nature* **276** (1978): 329-34.

46 **L.N. Plummer and T.M.L. Wigley** 'Dissolution of Calcite in CO<sub>2</sub>-Saturated Solutions at 25 degrees C and 1 Atmosphere Total Pressure', *Geochim. Cosmochim. Ac.* **40** (1976): 191-202; **L.N. Plummer et al.** 'Kinetics of Calcite Dissolution in CO<sub>2</sub>-Water Systems at 5-60 degrees C and 0.0-1.0 atm CO<sub>2</sub>', *Am. J. Sci.* **278** (1978): 179-216; **E.T. Sundquist et al.** 'Carbon-Dioxide in the Ocean Surface - Homogeneous Buffer Factor', *Science* **204** (1979): 1203-05.

combined with that of historical climatology, naturally led him to believe that global warming would provide a tremendous publicity source for the Unit. In 1978, reporting on the international conference on climate held in France, he wrote:

[W]e are still not absolutely certain that increased CO<sub>2</sub> will lead to warming. ... Still, the various models ... all predict warming, so the odds are strongly in favour of such an outcome and it is a possibility which must be taken with utmost seriousness.<sup>47</sup>

Lamb was uncomfortable with the new direction Wigley was heading in.<sup>48</sup> Other staff members of the younger generation thought differently. They all had a more quantitative background than Lamb. Kelly, Lamb's first PhD student who did his dissertation work on historical climatology under Lamb's supervision, had studied geophysics and meteorology in his undergraduate years at Reading. Phil D. Jones, who later took charge of the production of the global temperature data set, held a PhD in hydrology, one of the most mathematically oriented disciplines in the field of environmental sciences. Some of the members were more sympathetic to Lamb's qualitative approach than Wigley was,<sup>49</sup> but junior staff scientists generally welcomed this quantitative turn. On global warming, too, they shared Wigley's opinion, although for some time they did not particularly have the CO<sub>2</sub> problem in mind in carrying out their research. The construction of the global standard, land-based, gridded temperature data set required many years of painstaking data collection, assessing the quality of data, checking the consistency, and homogenization. It was only in the early 1980s that their temperature data were first formally published.<sup>50</sup> Yet, by 1979, with this common understanding and USDOE funding, the Unit's research activities were increasingly geared towards global warming research.<sup>51</sup>

47 **T.M.L. Wigley and P. Brimblecombe** 'What Is Climatology?' *Nature* **276** (1978): 213-14.

48 In his autobiography, he described this transition: "After only a few years [since his retirement] almost all the work on historical reconstruction of past climate and weather situations, which had first made the [Climatic Research] Unit well known, was abandoned." See **H.H. Lamb** *Through All the Changing Scenes of Life - A Meteorologist's Tale* (East Harling: Taverner, 1997)

49 **Kelly** *Interview by the author*

50 **P.D. Jones et al.** 'Variations in Surface Air Temperatures - Part 1. Northern Hemisphere, 1881-1980', *Mon. Weather Rev.* **110** (1982): 59-70; **P.M. Kelly et al.** 'Variations in Surface Air Temperatures - Part 2. Arctic Regions, 1881-1980', *Mon. Weather Rev.* **110** (1982): 71-83; **S.C.B. Raper et al.** 'Variations in Surface Air Temperatures: Part 3. The Antarctic, 1957- 82', *Mon. Weather Rev.* **112** (1984): 1341-53.

51 CRU's first research paper on CO<sub>2</sub>-induced global warming was submitted to *Nature* in mid 1979. **T.M.L.**

## Global Warming Begins to Emerge as an International Agenda

In February 1979, the World Meteorological Organization (WMO) held the First World Climate Conference (FWCC) in Geneva, with support from ICSU, UNEP and the Food and Agricultural Organization (FAO).<sup>52</sup> Compared to the usual scale of previous conferences and workshops on similar issues, this was a huge event and served as a major international forum on many different aspects of climate and its change. The first week of the Conference attracted over 350 scientists from more than 50 countries, including not just the western countries but also Russia, the Eastern Bloc, and some developing countries. Twenty-six overview papers were delivered and discussed on a range of climate-related topics. Many of the world-leading climate scientists participated in FWCC, along with the past and present heads of US, Russian and European meteorological services. There were a number of scientists from diverse specialties other than meteorology and climatology. During the second week, four working groups were formed, and more than 100 invited experts took part in detailed discussions to prepare the Conference Declaration and related documents.<sup>53</sup>

The planning of a large international climate conference originated from growing concerns over climate change in the mid 1970s. The idea was first broached at the 7<sup>th</sup> Congress of WMO in 1975 where the issue of climate change was widely discussed.<sup>54</sup> Two years later in 1977, in response to the suggestion of the Panel of Experts on Climate Change, the 29<sup>th</sup> session of the WMO Executive Committee requested that proposals for a long-term international effort to tackle the climate problem, the World Climate Programme (WCP), be drawn up in collaboration with other international agencies.<sup>55</sup> The Executive Committee also decided to convene a high-level conference, at which current knowledge would be reviewed and future plans examined. FWCC was the outcome of this process and, albeit attended mostly by scientists, quite explicitly dealt with the socio-political dimensions of climate change. One question raised for FWCC was whether an

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**Wigley et al.** 'Scenario for a Warm, High-CO<sub>2</sub> World', *Nature* **283** (1980): 17-21.

52 **WMO/UNEP/ICSU** *Proceedings of the World Climate Conference - A Conference of Experts on Climate and Mankind*, WMO series, No. 537 (Geneva: WMO, 1979)

53 **R.M. White** 'World Climate Conference - Report by the Conference Chairman', *WMO Bulletin* **28** (1979): 177-78.

54 **WMO** *Abridged Report with Resolutions - Seventh World Meteorological Congress*, WMO series, No. 416 (Geneva: WMO, 1975)

55 **WMO Executive Committee** *Abridged Report with Resolutions - Twenty-Ninth Session of the WMO*



international ministerial conference on climate change would be desirable, for which the participants concluded that more research was required before any policy decisions could be made.<sup>56</sup>

The FWCC actually devoted more time to natural climate variability. Climate was more often framed as a natural hazard or resource affecting human activities such as agriculture, fishing, forestry, hydrology and urban planning, rather than as fragile nature threatened by industrial development. However, the issue of human impact on global climate was not neglected at the Conference. In his keynote address, Robert M. White, chairman of the Conference and former head of NOAA, stated:

If natural climate disasters had not been enough to motivate governments and the scientific community to action, the ominous possibilities for man-induced climatic changes would have triggered our presence here.<sup>57</sup>

White emphasized the potential climatic consequences of increasing atmospheric CO<sub>2</sub>. He said:

By the addition of carbon dioxide to the atmosphere, we change its fundamental temperature controls. It is estimated that the burning of fossil fuels and destruction of forests ... have already, in the short span of one half century, increased atmospheric carbon dioxide content over 10 per cent. ... [T]he weight of scientific evidence predicts a significant global surface temperature increase.<sup>58</sup>

The CO<sub>2</sub>-climate connection was later scrutinized more closely in several of the overview papers and subsequent working group sessions.<sup>59</sup> The scientists involved in the discussions took a cautious stance, acknowledging a high degree of uncertainty, and did not call for any policy measures on global warming. Still, many of them generally postulated that the increase in atmospheric CO<sub>2</sub> might bring about long-term changes of

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*Executive Committee* (Geneva: WMO, 1977)

56 **A. Gilchrist** 'The World Climate Conference', *Weather* **34** (1979): 287-89.

57 **R.M. White** 'Climate at the Millennium - Keynote Address', in *Proceedings of the World Climate Conference* (Geneva: WMO, 1979). White was also chairman of the US NRC's newly established Climate Research Board at the time.

58 *Ibid.*

59 See, e.g., **B.J. Mason** 'Some Results of Climate Experiments with Numerical Models'; **R.E. Munn and L. Machta** 'Human Activities that Affect Climate'; **H. Flohn** 'A Scenario of Possible Future Climates, Natural and Man-Made'; and **J. Williams et al.** 'Energy and Climate: A Review with Emphasis on Global Interactions,' in *Proceedings of the World Climate Conference: A Conference of Experts on Climate and Mankind* (Geneva: WMO, 1979)

the global-scale climate. At the end of the Conference, while commending the WCP plan, the final Declaration suggested:

[W]e can say with some confidence that the burning of fossil fuels, deforestation, and changes of land use have increased the amount of carbon dioxide in the atmosphere ... It is likely that an increase will continue in the future. Carbon dioxide plays a fundamental role in determining the temperature of the earth's atmosphere, and it appears plausible that an increased amount of carbon dioxide in the atmosphere can contribute to a gradual warming of the lower temperature, especially at high latitude.<sup>60</sup>

The science behind this statement was not much updated from that presented in the preceding workshops organized by the National Academy of Sciences (NAS), SCOPE, IIASA or the Energy Research and Development Administration (ERDA, a predecessor of USDOE) in 1977-78.<sup>61</sup> FWCC nevertheless helped the issue of CO<sub>2</sub>-induced global warming reach a wider audience world-wide beyond the climate science community.

The proposed World Climate Programme (WCP) was approved in May at the 8<sup>th</sup> Congress of WMO and was officially instigated in 1980 under the joint auspices of WMO, UNEP and ICSU.<sup>62</sup> The WMO Research and Monitoring Project on Atmospheric Carbon Dioxide was accordingly integrated into its research component, the World Climate Research Programme (WCRP).<sup>63</sup> Throughout the early to mid 1980s, WCP acted as one of the basic frameworks for international assessments of the CO<sub>2</sub> question, which played a key role in drawing attention to the issue of global warming.<sup>64</sup> The role of WCP in promoting the science of global warming was, though, somewhat limited. Its main aim was to coordinate the climate-related research, monitoring and other activities of national

60 **WMO/UNEP/ICSU** 'The Declaration', in *Proceedings of the World Climate Conference* (Geneva: WMO, 1979)

61 **US NRC** *Energy and Climate* (Washington, DC: National Academy of Sciences, 1977); **B. Bolin et al.** (eds.) *The Global Carbon Cycle*, SCOPE 13 (Chichester: John Wiley & Sons, 1979); **W.P. Elliot and L. Machta** (eds.) *Workshop on the Global Effects of Carbon Dioxide from Fossil Fuels*, DOE/CONF-770385 (Washington, DC: US Department of Energy, 1979); **Williams** (ed.) *Carbon Dioxide, Climate and Society*. See also Chapter 4.

62 **WMO** *Outline Plan and Basis for the World Climate Programme, 1980-1983*, WMO Series, No. 540 (Geneva: WMO, 1980)

63 WCP was comprised of four components: (i) the World Climate Data Programme (WCDP); (ii) the World Climate Applications Programme (WCAP); (iii) the World Climate Impact Studies Programme (WCIP); and (iv) the World Climate Research Programme (WCRP). The main objectives of WCRP were: (a) to improve knowledge of global and regional climates; (b) to assess the evidence for significant trends in global and regional climates; (c) to develop and improve physical-mathematical models; and (d) to investigate the sensitivity of climate to possible natural and man-made influences. See *Ibid.*

64 **R.D. Bojkov** 'Summary of Some WMO Activity Relevant to Atmospheric Carbon Dioxide', in *Proceedings of the Carbon Dioxide Research Conference - Carbon Dioxide, Science and Consensus*, S.M. Kincade (ed.) (Washington, DC: US Department of Energy, 1982): A27-A38. See also below.

meteorological services and international agencies. No funding was provided for research through WCRP. Individual groups might get their research endorsed as a WCRP project, but had to find resources from funding bodies in their own countries.

It was in the US that the studies into global warming were most actively supported. There was a strong climate science community, with a number of research groups engaged in global warming research. Environmental problems were high on the public agenda. Perhaps more important in the context of the late 1970s was the perceived threat to energy security. From 1977, the eastern part of the US experienced three consecutive severe winters.<sup>65</sup> They might be expected to have weakened interest in global warming. On the other hand, by its nature, the question of the climatic effects of increasing atmospheric CO<sub>2</sub> was tied up with that of energy production. The widespread fear of an energy crisis in the US, reinforced by the increase in energy demand during the cold winters of the late 1970s, rather made the issue of global warming more visible. With this background, and the backing of the Carter administration, USDOE managed to secure a substantial amount of budget for CO<sub>2</sub> research programme and used it to fund various related projects within and outside the government.<sup>66</sup> In addition, when in 1978 the Congress enacted the National Climate Program Act, which originally grew out of the US government's anxiety about natural climate variability, USDOE, not NOAA, was assigned lead agency role in overseeing national research activities on atmospheric CO<sub>2</sub>.

The problem of global warming was especially highlighted in 1979 when the Iranian Revolution developed into the second oil shock, and an accident occurred at the nuclear power plant in Three Mile Island. In the face of the uncertain energy future, the Carter administration launched in July 1979 an ambitious \$ 88 billion decade-long programme for production of synthetic fuels derived from coal and oil shale.<sup>67</sup> The downside was that synthetic fuels were expected to release more CO<sub>2</sub> per unit energy, raising questions about the potential climatic consequences. Climate scientists could not reach a consensus on how serious those effects might be, but believed that there was a legitimate cause for

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65 See **Burroughs** *Does the Weather Really Matter?*

66 USDOE's CO<sub>2</sub> research programme was organized by the Carbon Dioxide Research Division of the Office of Basic Energy Sciences. This plan was originally formulated by W.W. Kellogg. See **US DOE Carbon Dioxide and Climate Division** *A Comprehensive Plan for Carbon Dioxide Effects Research and Assessment* (Washington, DC: US Department of Energy, 1980)

67 **T.R. Fehner and J.M. Holl** 'Part IV. The Carter Administration, 1977-1981', in *Department of Energy, 1977-1994 - A Summary History* (Washington, DC: US Department of Energy, 1994): 21-31.

concern. Soon after Carter's announcement, the White House Office of Science and Technology Policy (OSTP) asked NAS to undertake a formal scientific assessment on the effects of changes in atmospheric CO<sub>2</sub> on global climate.<sup>68</sup> Around the same time, Revelle, Keeling and their colleagues submitted a brief technical report to the US President's Council on Environmental Quality (CEQ), arguing that:

If we wait to prove that the climate is warming before we take steps to alleviate the CO<sub>2</sub> build-up, the effects will be well underway ... The potential disruptions are sufficiently great to warrant the incorporation of the CO<sub>2</sub> problem into all consideration of policy in the development of energy.<sup>69</sup>

The Senate Committee on Governmental Affairs likewise held a special one-day symposium on the very topic. Similar reservations about synthetic fuels were expressed by invited scientists, including Revelle, Schneider, Smagorinsky and Broecker.<sup>70</sup> As interest in the environmental effects of synthetic fuels was heightened, the need for global warming research came to be increasingly recognized. In FY 1980, USDOE's CO<sub>2</sub> research programme received a \$ 1 million budget rise, with the total reaching over \$ 6 million.<sup>71</sup>

The impact of the second oil shock, as with the first, was not restricted to the US but was global. The outbreak of the Iran-Iraq war in 1980 further disrupted already unstable international energy supplies. The average price of imported oil more than doubled between 1978 and 1980. In Britain, these events unfolded in the midst of the "winter of discontent". The winter of 1978-79 was exceptionally cold, among the severest in a century; the mean temperature of January and February was about 3.5 °C below the long-term average.<sup>72</sup> Politically, the period was marked by a series of strikes and mass

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68 **US NRC** *Carbon Dioxide and Climate - A Scientific Assessment* (Washington, DC: National Academy of Sciences, 1979); **F. Press** 'Science and Technology in the White House, 1977 to 1980 - Part 2.' *Science* **211** (1981): 249-56.

69 **G.M. Woodwell et al.** 'The Carbon Dioxide Problem: Implication for Policy in the Management of Energy and Other Resources - A Report to the Council on Environmental Quality', in *Carbon Dioxide Accumulation in the Atmosphere, Synthetic Fuels and Energy Policy - A Symposium* (Washington, DC: US GPO, 1979): 44-51.

70 **US Congress Senate Committee on Governmental Affairs** *Carbon Dioxide Accumulation in the Atmosphere, Synthetic Fuels and Energy Policy - A Symposium* (Washington, DC: US GPO, 1979)

71 **US DOE Carbon Dioxide Research Division** *The Carbon Dioxide Research Plan - A Summary* (Washington, DC: US Department of Energy, 1983); **K. Fisher-Vanden** *International Policy Instrument Prominence in the Climate Change Debate - A Case Study of the United States*, ENRP Discussion Paper E-97-06 (Cambridge: Kennedy School of Government, Harvard University, 1997)

72 **R. Heape** 'Winters of 1962/63 and 1978/79', *Weather* **36** (1981): 31-32.

demonstrations by public sector workers.<sup>73</sup> The sense of crisis was pervasive in the minds of the public, making the oil shock all the more depressing. But because Britain had for some time enjoyed sufficient energy generating capacity, and also because the British were preoccupied with the country's weak economic performance, energy concerns did not become as prevalent as in the US. Nor were there government plans for a major shift in energy sources. In contrast to what was happening in the US, the energy situation of the late 1970s did not prompt much discussion in Britain of CO<sub>2</sub>-induced global warming.<sup>74</sup>

This is not to say that Britain remained inactive on the issue of global warming. From around 1977, the Met Office began to accelerate its climate modelling research, of which the studies of CO<sub>2</sub>-induced global warming were an integral part. By the time FWCC was organized, where the director-general of Met Office John Mason presented an overview paper on climate modelling,<sup>75</sup> the Met.O.20 had its preliminary GCM results for doubled CO<sub>2</sub> and was carrying on its modelling work. The Interdepartmental Group on Climatology, established on Mason's initiative, was preparing climate impact assessments with emphasis upon the effects of increased CO<sub>2</sub>.<sup>76</sup> Outside the government, the Climate Research Unit at East Anglia went beyond the reconstruction of global land surface temperatures and developed temperature and precipitation scenarios for a "high-CO<sub>2</sub> world".<sup>77</sup> Apart from these activities, however, there were no other research groups pursuing global warming research. Policy aspects were rarely discussed, except by a few high-ranking scientists at the Met Office or in science media reports. Although CO<sub>2</sub>-induced global warming was clearly emerging as an important, international issue, each country as yet showed varying degrees of interest and support.

## Early Attempts to Forge a Consensus on Global Warming

Given the importance of fossil fuels to the economy, research into CO<sub>2</sub>-induced global

73 **K.O. Morgan** *Britain since 1945 - The People's Peace* (Oxford: Oxford University Press, 2001)

74 The CO<sub>2</sub> report of IEA Coal Research was one of the few exceptions. IEA Coal Research was a London-based working group of the International Energy Agency (IEA), which was founded in 1974 by member countries of the OECD. See **I.M. Smith** *Carbon Dioxide and the "Greenhouse Effect" - An Unresolved Problem*, Report No. ICTIS/ER01 (London: IEA Coal Research, 1978)

75 **Mason** 'Some Results of Climate Experiments with Numerical Models'.

76 **Interdepartmental Group on Climatology** *Climatic Change - Its Potential Effects on the United Kingdom and the Implications for Research* (London: HMSO, 1980)

77 See below.



warming entailed concerns about potential policy implications, though not always articulated. With rising interest in synthetic fuels, and with the USDOE's involvement, the link between scientific and policy discussions of global warming became more apparent in the US.

In April 1979, as a succession to the workshop held by ERDA in 1977, USDOE sponsored the American Association for the Advancement of Science (AAAS) to convene another workshop on the CO<sub>2</sub> issue.<sup>78</sup> The ERDA's 1977 workshop was primarily concerned with the carbon cycle and the effects of increasing CO<sub>2</sub> on climate. The USDOE-AAAS workshop, by contrast, broadened the scope to elucidate the "environmental and societal consequences". Experts were invited from the climate and CO<sub>2</sub> research as well as the biological and social sciences communities. They discussed the potential impacts that CO<sub>2</sub>-induced global warming could have on the ocean, the cryosphere, the biosphere and human society.<sup>79</sup> Uncertainty was perceived to be too great to permit the quantification of these impacts, but did not prohibit policy discussions. Instead, it was conceived to be crucial to identify areas of uncertainty where research priorities should be placed to provide a basis for sound policy decisions about the CO<sub>2</sub> issue.

The USDOE-AAAS workshop also stressed that, as a prerequisite for policy deliberations, scientists should agree upon a plausible scenario of the future climate due to increased CO<sub>2</sub>. The invited experts did not, though, attempt to confront this question at the workshop. They started with an assumed global warming scenario of 2–3 °C, adopting the most recent results of Manabe's GCM group at GFDL,<sup>80</sup> and focused upon the likely impacts of CO<sub>2</sub>-induced climate change. The task of forging a consensus on the global warming estimate was taken up a few months later by the NAS Ad Hoc Study Group on Carbon Dioxide and Climate, which was set up when, as noted above, the White House

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78 The chairman of the workshop was R. Revelle. **Carbon Dioxide Effects Research and Assessment Program Workshop on Environmental and Societal Consequences of a Possible CO<sub>2</sub>-Induced Climate Change** (Washington, DC: US Department of Energy, 1980)

79 The USDOE-AAAS workshop was organized into the following five panels: I. Environmental Effects on the Oceans, Cryosphere and Ocean Biota; II. Environmental Effects on the Less Managed Biosphere; III. Environmental Effects on the Managed Biosphere; IV. Social and Institutional Responses; and V. Issues Associated with Analysis of Economic and Geopolitical Consequences. See Ibid.

80 **T.M.L. Wigley** 'The Impact of Future Atmospheric CO<sub>2</sub> Increases', in *Workshop on Environmental and Societal Consequences of a Possible CO<sub>2</sub>-Induced Climate Change* (Washington, DC: US Department of Energy, 1980): 141–47. See also below.

OSTP requested NAS to review the CO<sub>2</sub> issue. A special four-day workshop was called in July 1979 to assess the scientific basis for projections of possible future climate changes resulting from fossil fuel emissions, and the degree of certainty that could be attached to these results.<sup>81</sup>

Chaired by a distinguished MIT dynamical meteorologist Jule Charney, the NAS Ad Hoc Study Group put significant weight on GCM results. At the time of the review process, there were three research groups around the world that had completed GCM simulations on the climatic effects of increased CO<sub>2</sub> – GFDL and NASA Goddard Institute of Space Studies (GISS) in the US and the Met Office in Britain.<sup>82</sup> The Study Group relied mainly on the work of the two US groups. Manabe group at GFDL had extended their 1975 study, first by incorporating a simplified form of cloud interaction into the original 9-level GCM, and secondly by using a new spectral GCM coupled with a simple mixed-layer ocean model.<sup>83-84</sup> These simulations gave annual mean surface warmings of *ca.* 3 and 2 °C for doubled CO<sub>2</sub>, respectively. At NASA GISS, after a few climate sensitivity experiments, James Hansen and his colleagues commenced GCM studies of the greenhouse effect.<sup>85</sup> While GISS's 7-level GCM had coarse resolution and was in its development stage, the model provided preliminary warming estimates of 3.5

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81 The members of the Ad Hoc Study Group included some of the most renowned dynamical meteorologists, atmospheric physicists, and physical oceanographers. They were J.G. Charney (MIT), A. Arakawa (UCLA), J. Baker (University of Washington), B. Bolin (University of Stockholm), R.E. Dickinson (NCAR), R.M. Goody (Harvard), C.E. Leith (NCAR), H.M. Stommel (Woods Hole Oceanographic Institution), and C.I. Wunsch (MIT). **US NRC Carbon Dioxide and Climate - A Scientific Assessment**.

82 A group led by W.L. Gates, who moved from RAND to Oregon State University in 1976, was also beginning to employ a GCM to study CO<sub>2</sub>-induced global warming. But its first result came out after the final report of the NAS Study Group was prepared.

83 The original GFDL GCM had fixed clouds, idealized geography, and swamp ocean. For the inclusion of cloud feedback, see **S. Manabe and R.T. Wetherald** 'On the Distribution of Climate Change Resulting from an Increase in CO<sub>2</sub> Content of the Atmosphere', *J. Atmos. Sci.* **37** (1980): 99-118. As noted in Chapter 4, GFDL's simulations using the original and modified finite-difference GCMs were both confined to the Northern Hemisphere.

84 A new spectral GCM, global in scope, again had fixed clouds but had realistic geography. The coupling of a simple mixed layer ocean model also allowed upper ocean layers to store and release heat locally, so that seasonal variations could be incorporated into simulations. However, like the swamp ocean approach, it neglected horizontal heat transport by the oceans. In addition, the transfer of heat to lower deep ocean layers was still ignored, which was unrealistic for time-scales longer than several decades. See **S. Manabe and R.J. Stouffer** 'CO<sub>2</sub> Climate Sensitivity Study with a Mathematical-Model of the Global Climate', *Nature* **282** (1979): 491-93; 'Sensitivity of a Global Climate Model to an Increase of CO<sub>2</sub> Concentration in the Atmosphere', *J. Geophys. Res.* **85** (1980): 5529-54.

85 Hansen and his colleagues were very keen to develop GCM-based global warming research, but the progress was rather slow. One of the reasons was that Robert Jastrow, director of NASA GISS, wanted them to focus on the "farmer's forecast". **J. Hansen et al.** 'Climate Modeling in the Global Warming Debate', in *General Circulation Model Development - Past, Present, and Future*, D.A. Randall (ed.) (San Diego: Academic Press, 2000): 127-64. As already noted, Jastrow did not regard highly the study of human impact on climate and later became a global warming sceptic.

and 3.9 °C (swamp and mixed-layer ocean) that could be directly compared to those from GFDL.<sup>86</sup>

From the results from GFDL and NASA GISS, the Study Group estimated “the most probable global warming for a doubling of CO<sub>2</sub> to be near 3 °C with a probable error of  $\pm$  1.5 °C”.<sup>87</sup> Based on a qualitative evaluation of model characteristics and feedback mechanisms, the members of the Study Group accepted 2 and 3.5 °C as the lower and upper bounds, excluding 3.9 °C. Different margins of error were then allowed on each side, and the middle was chosen as the most probable value. These collective judgments were no less informal and intuitive than Schneider’s derivation of a “state-of-the-art order-of-magnitude estimate” of 1.5–3 °C from RCM results.<sup>88</sup> The choice of the lower and upper bounds also contained more arbitrariness than it might first appear. When later asked about his GCM results used in the Charney report, Manabe recalled, “I’ve had all kinds of numbers, but it just happened to be 2 °C at the time”.<sup>89</sup> Yet, the need for delivering policy-relevant, quantitative estimates in a limited period of time seems to have made this type of discretionary practice acceptable.

It was because of this focus on global warming estimates that the Charney report did not discuss the work of John Mitchell at the Met.O.20. Mitchell’s results were available to the NAS Study Group.<sup>90</sup> Since these were the first GCM results that simulated the seasonal and geographical distribution of the earth’s radiative response to doubled CO<sub>2</sub>, and the Met.O.20 GCM had a better horizontal resolution and more sophisticated parameterization schemes, one could argue that they should have been considered if the climatic effects of increased CO<sub>2</sub> were to be understood as a whole.<sup>91</sup> However, as suggested in Chapter 4, in order to include the seasonal and regional variations, Mitchell had to prescribe sea surface temperatures and the sea-ice distribution in his model. These imposed conditions put a strong constraint on the changes of the model surface temperatures, whereas it was these changes that the NAS Study Group wanted to

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86 Hansen’s GCM had a coarser horizontal resolution of 8° x 10° (latitude/longitude) than the Met.O.20 (3° x 5°) and GFDL (4.5° x 5°) models. **US NRC** *Carbon Dioxide and Climate - A Scientific Assessment*.

87 Ibid.

88 **S.H. Schneider** ‘On the Carbon Dioxide-Climate Confusion’, *J. Atmos. Sci.* **32** (1975): 2060-66.

89 **W.K. Stevens** *The Change in the Weather - People, Weather, and the Science of Climate* (New York: Delacorte Press, 1999)

90 **US NRC** *Carbon Dioxide and Climate - A Scientific Assessment*

91 For J.F.B. Mitchell’s first CO<sub>2</sub> simulation, see **Mitchell** *Preliminary Report on the Numerical Study of the Effect*

concentrate on.<sup>92</sup>

In Britain, as the Met.O.20's modelling illustrated, there was no pressure for producing the consensus estimate for climate sensitivity. The British government did not ignore the environmental consequences of energy production. In late 1978, the Department of the Environment set up the Standing Commission on Energy and the Environment to look into the interaction between energy policy and the environment. The Commission initiated a comprehensive study of the environmental implications of the extraction, production and use of coal.<sup>93</sup> But its main concern was acid rain and other pollution issues. Neither the Commission nor the Department of Energy took an interest in the climatic effects of fossil fuel emissions. From 1979 onwards, the Research Division of the Central Electricity Generating Board (CEGB) appointed an atmospheric physicist as a full-time research officer, but merely to keep a watching brief on the CO<sub>2</sub> issue.<sup>94</sup> The Met Office was the only government institution conducting investigations into the greenhouse effect, and these were part of the bigger effort to develop global climate models.

Some discussions about policy aspects of global warming took place through the Interdepartmental Climatology Group. Yet even there, no urgency was felt to come up with the formal estimates of global warming. This small liaison committee was chaired by an economist Sir Kenneth Berrill, head of the Cabinet Office's Central Policy Review Staff, and comprised of scientists and staff members from nine government departments and agencies, the most influential of whom was John Mason, director-general of the Met Office.<sup>95</sup> The aim of the committee was confined to reviewing the implications of advances in climate science, including global warming research, for the government

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*on Climate of Increasing Atmospheric Carbon Dioxide .*

92 As discussed in Chapter 4, to tackle this issue, the Met.O.20 imposed a 2 °C increase on the sea surface temperatures everywhere for doubled CO<sub>2</sub> (based on the simple climate model results) and carried out a further integration. The final calculation gave an average surface warming of ca. 2.7 °C. However, this result was not made available to the NAS Study Group. See **British Met Office Annual Report, 1979**. W.L. Gates' group at OSU took a similar modelling strategy although its 2-level GCM was much simpler than the Met.O.20 GCM. See **W.L. Gates** 'Modeling the Surface Temperature Changes due to Increased Atmospheric CO<sub>2</sub>', in *Interactions of Energy and Climate - Proceedings of an International Workshop*, W. Bach et al. (eds.) (Dordrecht: D. Reidel, 1980): 169-90.

93 **Commission on Energy and the Environment Review of Activities 1978-79** (London: HMSO, 1979)

94 **J. Sheail** *Power in Trust - The Environmental History of the Central Electricity Generating Board* (Oxford: Clarendon Press, 1991)

95 Other members included M.W. Holdgate (director-general of research, Department of the Environment), R.J.H. Beverton (secretary, NERC), Sir Hermann Bondi (chief scientist, Department of Energy), JM Ashworth (chief scientist, Central Policy Review Staff), DS Davies (chief scientist, Department of Industry), and also the staff members from the Foreign and Commonwealth Office, the Ministry of Defence, and the Ministry of Agriculture, Fisheries and Food. See **Interdepartmental Group on Climatology** *Climatic Change* .

departments and agencies and, more generally, for the British economy. The assessment of the scientific aspects of climate change was based upon informal judgments of the Met Office senior scientists.

The Met Office scientists believed that GCM results were not refined enough to be used as a basis for policy formulation.<sup>96</sup> When they were asked to provide quantitative estimates, they gave a guarded response. In January 1980, the Interdepartmental Climatology Group published a short report on climate change via the Cabinet Office.<sup>97</sup> The report based its discussions on the warming estimate for a doubling of CO<sub>2</sub> concentrations. Interestingly, a more modest warming estimate of 1–2 °C was given, despite the fact that the Met Office scientists were aware of the final report of the NAS Study Group.<sup>98</sup> With these relatively moderate climate changes, the tone of the report was fairly optimistic. Concern was expressed about the potential increase in the frequency of extreme weather as a result of global warming, but it maintained that the warming of 1–2 °C, and accompanying changes in rainfall, would create no major problems for Britain. The report even implied that global warming could be beneficial in some respects when it wrote that “each rise of 1 degree C in mean annual temperature would decrease annual energy demand by about 2 percent, saving £ 250 million a year”.<sup>99</sup> The Group members were content with the Met Office’s organization of climate research and concluded that:

because modest climate changes are not expected to have great impact on the United Kingdom’s economy, we see no need for major new co-ordinating machinery or for a formal United Kingdom programme on climatology.<sup>100</sup>

This optimistic, British centered, conclusion was in contrast to that of *The Global 2000 Report to the President*, published in the same month (January 1980) by CEQ in the US.<sup>101</sup> Amongst a range of topics from food, energy and resource prices to species extinctions, global climate change was taken as one of the potentially most critical environmental problems. Although the Global 2000 Report mentioned the possibility of

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96 **Mason** *Interview by the author*

97 **Interdepartmental Group on Climatology** *Climatic Change* .

98 The report did not give the details as to how the estimate of 1-2 °C was inferred. *Ibid*.

99 *Ibid*.

100 *Ibid*.

101 **Council on Environmental Quality** *The Global 2000 Report to the President - Entering the Twenty-First Century* (Washington, DC: US GPO, 1980). The Global 2000 Report was later republished in five languages and more than half a million copies were sold around the world.



global cooling, it devoted considerable space to the issue of CO<sub>2</sub>-induced global warming. The NAS' estimate of 1.5–4.5 °C was not yet widely used, and as in the USDOE-AAAS workshop, CEQ adopted the assumption that a doubling of the atmospheric CO<sub>2</sub> content by 2050 would lead to a 2–3 °C rise in global mean temperature. Even taking into account that the difference between the estimates of 1–2 °C and 2–3 °C was not trivial, its conclusion was gloomy. It warned that global warming might give rise to substantial alterations in weather and precipitation patterns in the temperate zones, where much of the world's food production was located, and would coincide with deteriorating conditions in agriculture, forests and other resources.<sup>102</sup>

These results were more or less repeated a year later in CEQ's major CO<sub>2</sub> report, *Global Energy Futures and the Carbon Dioxide Problem*.<sup>103</sup> Thus, the consensus about global warming was far from global.

### Scepticism towards Global Warming

It should be noted that the Met Office did not argue specifically for lower estimates of global warming. For the Office, it was imperative to refrain from conveying an alarmist message to the policy community and the public in the absence of a concrete scientific basis. While some US scientists thought that “a wait-and-see policy may mean waiting until it is too late”,<sup>104</sup> the Met Office scientists' view was that caution constituted proper conduct of scientific practice. Occasionally, however, this conservative style of science seems to have translated into a scientific understanding that was positively sceptical of high estimates for global warming. Mason, for example, was dubious about the way the issues of anthropogenic climate change were often addressed in the US. In his presentation on climate modelling at FWCC, he claimed that the early US predictions of stratospheric ozone decrease by SSTs were “exaggerated”.<sup>105</sup> He then suggested that, as the SST case showed, the atmosphere might be more resilient than commonly assumed,

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<sup>102</sup> Ibid.

<sup>103</sup> **Council on Environmental Quality** *Global Energy Futures and the Carbon Dioxide Problem* (Washington, DC: US GPO, 1981)

<sup>104</sup> See the Foreword written by V.E. Suomi, Chairman of the NAS Climate Research Board in **US NRC** *Carbon Dioxide and Climate - A Scientific Assessment*.

<sup>105</sup> **Mason** 'Some Results of Climate Experiments with Numerical Models'.

and that the warming effects of increased CO<sub>2</sub> might well be offset by natural, negative feedback mechanisms.<sup>106</sup>

Such a position may have been Mason's own rather than representing the views of other Met Office scientists. Whether or not this was the case, Mason and his staff at the Met Office took global warming research seriously. They recognized early on that the issue of CO<sub>2</sub>-induced global warming was of increasing political and economic importance internationally, and believed that it deserved the Office's research interest. Mason, notwithstanding his conservative stance, enthusiastically endorsed the launch of GCM studies into the greenhouse effect at the Me.O.20, overseen by deputy director of dynamical research Andrew Gilchrist.<sup>107</sup> One reason for establishing the Met.O.20's climate modelling sub-branch was to strengthen research in this area. Simultaneously, Mason helped to raise the scientific and policy concerns about the CO<sub>2</sub> issue within the British government.<sup>108</sup> It was as a result of his idea that, through the Interdepartmental Group on Climatology, government departments and agencies, for the first time in Britain, conducted climate impact assessments for a global warming scenario.<sup>109</sup>

Ironically, the report of the Interdepartmental Group was published shortly after the Conservative Party led by Margaret Thatcher came to power, with its strong anti-government ideology and scepticism towards environmental agenda. "The time could hardly have been less propitious", as Gilchrist later recalled.<sup>110</sup> The modest assessments by the Group did not include any policy recommendations for the British government. The only proposal it made was that the state of progress in climate research and its policy implications be reviewed annually from early 1980.<sup>111</sup> Even this "was treated dismissively by the relevant ministers."<sup>112</sup> After publishing its first report, the Interdepartmental Group ceased to exist. The government's annual review of climate research never materialized. In the US, too, Reagan's new government with a strong conservative bent came into office

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106 Mason argued that a change of only 1 percent in the total cloud cover could mask the effects of a 25 percent increase in CO<sub>2</sub>. See *Ibid.* Mason's conservative stance, perhaps more clearly stated in an oral version of his presentation, annoyed S.H. Schneider, who was also present at FWCC. Schneider described his encounter with Mason at FWCC in *S.H. Schneider Global Warming - Are We Entering the Greenhouse Century?* (San Francisco: Sierra Club, 1989)

107 **Gilchrist** *Interview by the author*. See also Chapter 4.

108 *Ibid.*

109 **Mason** *Interview by the author*.

110 **Gilchrist** *Interview by the author*.

111 **Interdepartmental Group on Climatology** *Climatic Change*.

in early 1981. The reports on the CO<sub>2</sub> issue by Carter's CEQ were ignored. The Reagan administration also started slashing the federal budget. USDOE was no exception, and its R&D expenditure was cut by more than half.<sup>113</sup> The CO<sub>2</sub> research programme, especially those elements dealing with societal and environmental consequences, was hit hard.<sup>114</sup>

These changes were not as damaging to advances in the science of global warming as one would expect. In Britain, the Met Office had retained the autonomy to govern its own research activities. The change of power in 10 Downing Street had little, if any, impact on the Office's global warming research. In late 1979, the Office managed to order a new powerful supercomputer Cyber 205 from Control Data Corp., which was installed in early 1981 and boosted the Met.O.20's climate modelling research.<sup>115</sup> In the US, as mentioned, the government's research efforts on global warming suffered a setback. And yet, the Democrat-controlled Congress was more sympathetic to such work.<sup>116</sup> Though not as ambitious as before, USDOE's Carbon Dioxide Research Division was able to coordinate and fund various research projects on CO<sub>2</sub>-induced climate change.<sup>117</sup> Moreover, unlike in Britain where climate research was dominated by the Met Office, and to a lesser extent CRU, there were diverse research constituencies to keep global warming research afloat.

Further support to the CO<sub>2</sub> research communities came from outside Britain and the US. In late 1980, the World Meteorological Organization (WMO), the United Nations Environment Programme (UNEP) and the International Council of Scientific Unions (ICSU) held the first joint meeting in Villach, Austria on the role of CO<sub>2</sub> in climate

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112 **Gilchrist** *Interview by the author*.

113 **Fisher-Vanden** *International Policy Instrument Prominence in the Climate Change Debate*.

114 **S.H. Schneider** *Interview by the author* (January 23, 2001).

115 **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1980-81); **B.J. Mason** *Interview by R.J. Ogden* (Royal Meteorological Society Interview: June 4, 1985). Of course, climate modelling research was not the only reason for the procurement of the Cyber 205. Operational weather forecasting and the development of weather forecast models remained the Met Office's most important institutional goals. But the Cyber 205 computer was 40 times faster in numerical modelling computations than the IBM 360/195, allowing the Office to carry out these researches simultaneously.

116 Some of the congressional Democrats, including Representative Albert Gore Jr., waged a battle with the Reagan administration over the budget cut in USDOE's CO<sub>2</sub> research programme. **Fisher-Vanden** *International Policy Instrument Prominence in the Climate Change Debate*. See also **US Congress House Committee on Science and Technology** *Carbon Dioxide and Climate: The Greenhouse Effect - Hearing before the Subcommittee on Natural Resources, Agriculture Research, and the Environment* (Washington, DC: US GPO, 1982). It is well known that Gore first learned about the issue of global warming when he attended Roger Revelle's course as a freshman at Harvard in 1965. See **A. Gore** *Earth in the Balance - Ecology and the Human Spirit* (Boston: Houghton Mifflin, 1992).

117 **US DOE Carbon Dioxide Research Division** *The Carbon Dioxide Research Plan*.

variations and its associate impacts.<sup>118</sup> Its assessment, calling for more research, was not strong enough to attract the attention of conservative British and US administrations. As part of WCP, the meeting nonetheless gathered many influential climate scientists across the globe, and the final report was widely distributed internationally by the three organizations. A sequence of follow-up meetings, conferences and reports ensued in 1981-83.<sup>119</sup> At the European level, the Commission of the European Communities embarked on a 5-year climatology programme in 1980, in which some funding was allocated to global warming research.<sup>120</sup> Bert Bolin at Stockholm continued to take a prominent part in ICSU's SCOPE programme on the global carbon cycle.<sup>121</sup> In West Germany, Wilfrid Bach at the University of Münster and Jürgen Pankrath of the Federal Environmental Agency became active on the CO<sub>2</sub> issue, organizing several international workshops.<sup>122</sup> All these developments contributed to airing the international scientific concerns about CO<sub>2</sub>-induced climate change.

In a conservative political climate, however, scientists came under stronger pressure for achieving a high level of scientific certainty on environmental matters. This was more so in the US where policy culture was adversarial and formal<sup>123</sup> and more diverse groups participated in global warming research. The remark made by Democratic representative Albert Gore Jr., in a 1982 congressional hearing on the CO<sub>2</sub> issue, exemplified the atmosphere:

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118 **WMO/UNEP/ICSU** *Report of the WMO/UNEP/ICSU Meeting of Experts on the Assessment of the Role of CO<sub>2</sub> on Climate Variations and Their Impact* (Geneva: WMO, 1981)

119 **WMO/UNEP/ICSU** *Papers presented at the WMO/UNEP/ICSU Scientific Conference on Analysis and Interpretation of Atmospheric CO<sub>2</sub> Data* (Geneva: WMO, 1981); *Report of the WMO/UNEP/ICSU Meeting on Instruments, Standardization and Measurement Techniques for Atmospheric CO<sub>2</sub>* (Geneva: WMO, 1982); **WMO** *Report of the Meeting of Experts on Detection of Possible Climate Change* (Geneva: WMO, 1982); *An Assessment of BAPMoN Data Currently Available on the Concentration of CO<sub>2</sub> in the Atmosphere* (Geneva: WMO, 1982); *Report of the Meeting of Experts on the CO<sub>2</sub> Concentrations from Pre-industrial Times to I.G.Y.* (Geneva: WMO, 1983). For a brief summary for 1980-82, see **Bojkov** 'Summary of Some WMO Activity Relevant to Atmospheric Carbon Dioxide'.

120 **R. Fantechi and A. Ghazi** *First R&D Programme in the Field of Climatology, 1981-85* (Luxembourg: Commission of the European Communities, 1985). IIASA and NATO Advanced Study Workshop also played an important role.

121 **B. Bolin** (ed.) *Carbon Cycle Modelling*, SCOPE 16 (Chichester: John Wiley & Sons, 1981); **B. Bolin and R.B. Cook** (eds.) *The Major Biogeochemical Cycles and Their Interactions*, SCOPE 21 (Chichester: John Wiley & Sons, 1983)

122 **W. Bach et al.** (eds.) *Man's Impact on Climate - Proceedings of an International Conference* (Amsterdam: Elsevier, 1979); *Interactions of Energy and Climate - Proceedings of an International Workshop* (Dordrecht: D. Reidel, 1980); *Food-Climate Interactions - Proceedings of an International Workshop* (Dordrecht: D. Reidel, 1981); *Carbon Dioxide - Current Views and Developments in Energy/Climate Research* (Dordrecht: D. Reidel, 1983)

123 **S. Jasanoff** 'Acceptable Evidence in a Pluralistic Society', in *Acceptable Evidence - Science and Values in Risk Management*, D.G. Mayo and R.D. Hollander (eds.) (Oxford: Oxford University Press, 1991): 27-47.

[I]t does seem to me that if we can elevate the degree of certainty, we will have a better chance of summoning up the political will to address this problem.<sup>124</sup>

For those who adopted this line of reasoning, it was paradoxical that dissenting minority views became more articulated as political pressure for scientific certainty mounted in the US.

Bryson kept insisting that the effects of aerosols would be important and would reduce climate sensitivity to CO<sub>2</sub> concentrations.<sup>125</sup> But doubts about the global warming hypothesis also came from other scientific groups. In mid 1979, Reginald Newell at MIT and Thomas Dopplick at the Air Force examined the changes in tropical sea surface temperature due to a doubling of CO<sub>2</sub>, using a static radiative flux model based on an empirical representation of the surface energy balance. They pointed out that the temperature changes predicted by GCMs and RCMs arose predominantly from additional water vapour evaporated into the atmosphere. According to their calculations, the direct radiative effects of doubled CO<sub>2</sub> on surface temperature would be less than 0.25 K.<sup>126</sup> From 1980 onwards, Sherwood Idso at the US Water Conservation Laboratory began to evaluate the global warming hypothesis. He empirically deduced the parameter relating a change in infrared flux at the surface to a corresponding change in surface temperature, and with this value from “natural experiments”, calculated a similarly low warming estimate of  $\geq 0.26$  K for doubled CO<sub>2</sub>.<sup>127</sup>

These scientists were from different sub-disciplinary backgrounds within the field of meteorology. They had yet something in common – empirical observations were integral to their studies. Newell and his former student Dopplick were in the tradition of Victor Starr’s diagnostic studies of the general circulation at MIT, what might be categorized as “the observational branch of dynamical climatology”.<sup>128</sup> Idso was an agricultural-physical meteorologist who had extensively used local observations to investigate atmospheric

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124 Quoted from **Fisher-Vanden** *International Policy Instrument Prominence in the Climate Change Debate*.

125 'Discussion Period on Climate Modeling', in *Proceedings of the Carbon Dioxide and Climate Research Program Conference*, L.E. Schmitt (ed.) (Washington, DC: US Department of Energy, 1980): 133-62.

126 **R.E. Newell and T.G. Dopplick** 'Questions Concerning the Possible Influence of Anthropogenic CO<sub>2</sub> on Atmospheric Temperature', *J. Appl. Meteorol.* **18** (1979): 822-25. It is noteworthy that this work was supported by USDOE.

127 **S.B. Idso** 'Climatological Significance of a Doubling of Earth's Atmospheric Carbon Dioxide Concentration', *Science* **207** (1980): 1462-63

128 See Chapter 2.



radiation balance. While both groups quoted the work of palaeoclimatologists to back up their results,<sup>129</sup> they did not necessarily share Bryson's actuarial approach to past climates. Their views on the radiative effects of aerosols were not quite the same as Bryson's, either.<sup>130</sup> Yet, they, and Bryson, all had sceptical views on the deductive approach of GCM modellers. Idso was open about this, declaring that there was a "philosophical difference" between modellers' approach and his experimentalist one.<sup>131</sup>

Questions of a different kind were raised by Richard S. Lindzen at Harvard. Lindzen, Richard Goody's former student, was a well-known dynamical meteorologist and in 1983 succeeded to Charney's chair of meteorology at MIT. He conceded that numerical modelling was the best available option for atmospheric scientists. Even so, he was very critical of a GCM version of this endeavour, which in his view could not incorporate detailed physical and dynamical processes.<sup>132</sup> Lindzen opted for a distinctive strategy. In 1981, he and his students employed simple climate models to test the effects of the choice of convective parameterization on the model's climate sensitivity to CO<sub>2</sub> increase. The model with a physically-based cumulus convective parameterization appeared to have a smaller sensitivity, yielding a global increase of 1.3 K for doubling the CO<sub>2</sub> content but much less at low latitudes. From these results, they insinuated that cumulous convection in the real climate might inhibit a strong positive water vapour feedback, and in turn the "possibility of a runaway greenhouse on earth".<sup>133</sup>

Later in the 1990s, Lindzen became an outspoken global warming sceptic. In the early 1980s, his results were more in tune with mainstream climate science.<sup>134</sup> It was the studies

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129 **R.E. Newell and T.G. Dopplack** 'Reply to Robert G. Watts', *J. Appl. Meteorol.* **20** (1981): 114-17; **S.B. Idso** 'Carbon Dioxide - an Alternative View', *New Sci.* **92** (1981): 444-46.

130 They accepted that the increase in stratospheric dust by volcanic eruptions would cool the climate, but that the injection of anthropogenic dust into the lower troposphere would lead to a surface warming. Bryson, on the other hand, maintained that the increase in anthropogenic aerosols would have a cooling effect. In 1977, Idso criticized such a view: "... whereas many groups ... have looked on this aspect of intensified industrialization as acting as a 'brake' on the warming influence of increased carbon dioxide production, just the opposite is actually the case ...". See **S.B. Idso and A.J. Brazel** 'Planetary Radiation Balance as a Function of Atmospheric Dust - Climatological Consequences', *Science* **198** (1977): 731-33.

131 **Idso** 'Carbon Dioxide - an Alternative View'.

132 Schneider recalled that, as early as in 1978, Lindzen was candid about his disdain for the use of GCMs. **Schneider** *Interview by the author*.

133 **R.S. Lindzen et al.** 'The Role of Convective Model Choice in Calculating the Climate Impact of Doubling CO<sub>2</sub>', *J. Atmos. Sci.* **39** (1982): 1189-205.

134 In fact, his findings were not so different from the results given by Rowntree and Walker at the Met.O.20 in 1978. See **Rowntree and Walker** 'The Effects of Doubling the CO<sub>2</sub> Concentration on Radiative-Convective Equilibrium'. See also Chapter 4.

by Newell, Dopplnick and Idso that sparked a series of scientific debates.<sup>135</sup> Most climate modellers readily admitted that the direct radiative effects of increased CO<sub>2</sub> would be small. What they rejected was an implicit claim that the amplifying effect of water vapour feedback might have been grossly overestimated in GCM and RCM studies of the greenhouse effect. One member of the NAS Study Group was quoted as saying that Newell, Dopplnick and Idso's studies were "based on incomplete assessments that unrealistically omit important feedback processes."<sup>136</sup> If the positive water vapour feedback was properly considered, the majority view asserted, the small direct effects of CO<sub>2</sub> were nothing unusual and were consistent with the proposed warming estimates. Idso's study was regarded as particularly problematic. Many found his "natural experiments" unsuitable for studying the global processes since they were essentially observations from a few local sites.<sup>137</sup>

Perhaps what most upset the US climate research community was Idso's attack on the validity of GCMs and its use in policy setting. He wrote:

There ... appears to be a major discrepancy between current theory and experiment relative to the effects of carbon dioxide on climate. Until this discrepancy is resolved, we should not be too quick to limit our options in the selection of future energy alternatives.<sup>138</sup>

[B]oth the experimentally observed response characteristics of the real atmosphere and real climatic history cast doubts upon predictions of general circulation models that yield mean global temperature increases of 2 to 4 K for a doubling of the atmospheric CO<sub>2</sub> content.<sup>139</sup>

Lindzen did not yet overtly voice his opinion as Idso did. In private, in addition to being distrustful of GCMs, he was disapproving of the way climate science and policy

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135 For the debates on Newell & Dopplnick (1979), see **R.G. Watts** 'Discussion of "Questions Concerning the Possible Influence of Anthropogenic CO<sub>2</sub> on Atmospheric Temperature"', *J. Appl. Meteorol.* **19** (1980): 494-95; **Newell and Dopplnick** 'Reply to Robert G. Watts'; **R.G. Watts** 'Further Discussion of "Questions Concerning the Possible Influence of Anthropogenic CO<sub>2</sub> on Atmospheric Temperature"', *J. Appl. Meteorol.* **21** (1982): 243-47; 'Discussion Period on Climate Modeling'. For the debates on Idso's work, see **S.H. Schneider et al.** 'Carbon Dioxide and Climate', *Science* **210** (1980): 6-7; **S.B. Idso** 'Reply - Carbon Dioxide and Climate', *Science* **210** (1980): 7-8. See also **V. Ramanathan** 'The Role of Ocean-Atmosphere Interactions in the CO<sub>2</sub> Climate Problem', *J. Atmos. Sci.* **38** (1981): 918-30.

136 **Anon.** 'CO<sub>2</sub> Could Increase Global Tensions', *Nature* **285** (1980): 61.

137 **R.A. Kerr** 'CO<sub>2</sub>-Climate Models Defended', *Science* **217** (1982): 620.

138 **Idso** 'Climatological Significance of a Doubling of Earth's Atmospheric Carbon Dioxide Concentration'.

139 **Idso** 'Reply - Carbon Dioxide and Climate'.

interacted in the US. Despite his involvement in CIAP and NAS Study Group, Lindzen was convinced that science should be curiosity-driven and never be done according to the political time scale.<sup>140</sup> These criticisms made mainstream climate scientists worry that “policy makers were getting confused”<sup>141</sup> and feel obliged to clarify the issue. When the Congress and OSTP asked NAS to reassess the CO<sub>2</sub> issue in framing the Energy Security Act in 1981,<sup>142</sup> NAS’ CO<sub>2</sub>/Climate Review Panel chaired by Joseph Smagorinsky decided to add rebuttal comments in the final report. Some members of the Panel were opposed to this idea on the grounds that it could dignify their arguments. But Stephen Schneider, as an invited expert to the panel, pushed hard for an official rebuttal.<sup>143</sup>

The British situation was different. In a more consensual and informal policy culture, the problems of scientific certainty, and of difference of opinion, were dealt with in a closed manner inside the expert community.<sup>144</sup> The Met Office’s dynamical climatology research occupied a dominant position in British climate science. The scepticism towards the use of GCMs, uttered by Hubert Lamb,<sup>145</sup> was by and large dismissed by the Office without causing more scientific debate. As for the global warming hypothesis, the Met Office had already been taking a cautious, wait-and-see approach, and did not need to be at odds with the Conservative government or dissenting scientists such as Lamb. Within the Met Office, a hierarchical culture made it difficult for the Office scientists to openly express a divergent view from the official one, or publicly engage in scientific disputes that could have implications for policy. Not surprisingly, there was no highly publicized controversy over the CO<sub>2</sub> issue in Britain. The only British scientist who formally published on the work of Newell, Dopplack, and Idso was A.J. Crane, a research scientist

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140 **Schneider** *Interview by the author*. Lindzen was a member of the CIAP’s panel on stratospheric modelling during the early to mid 1970s. He was also invited to participate in the discussion by NAS Study group in 1979. See **R.E. Dickinson et al.** ‘Modeling of the Natural Stratospheric Climate’, in *The Natural Stratosphere of 1974* (Washington, DC: US Department of Transportation, 1975); **US NRC** *Carbon Dioxide and Climate - A Scientific Assessment*.

141 **S. Budiansky** ‘Academy Rebuts Idso on CO<sub>2</sub> Research’, *Nature* **298** (1982): 411.

142 **US NRC** *Carbon Dioxide and Climate: A Second Assessment* (Washington, DC: National Academy of Sciences, 1982)

143 **Schneider** *Interview by the author*; **Budiansky** ‘Academy Rebuts Idso on CO<sub>2</sub> Research’. See also **US NRC** *Carbon Dioxide and Climate: A Second Assessment*.

144 For the discussion of different styles of science in different policy cultures, see, e.g., **S. Jasanoff** *Risk Management and Political Culture - A Comparative Study of Science in the Policy Context* (New York: Russell Sage Foundation, 1986); **Jasanoff** ‘Acceptable Evidence in a Pluralistic Society’; **O. Renn** ‘Style of Using Scientific Expertise: A Comparative Framework’, *Science and Public Policy* **22** (1995): 147-56.

145 See Chapter 3.

at CEEB, and he defended the majority view.<sup>146</sup>

## Maturing the Science of Global Warming in Britain

### *The Meteorological Office*

The new Cyber 205 computer having been installed, the Met.O.20 directed extensive efforts to improve the 5- and 11-level GCMs. For example, numerical integrations were run to determine the model's sensitivity to different parameterizations of land-surface or soil moisture. In order to check the physical realism of the models, the short- and long-term variabilities in simulation results were analysed.<sup>147</sup> The Met.O.20 aimed to extend these models to a full climate model that would include explicit and interactive representation of clouds, oceans and sea-ice, rather than characteristics prescribed by present-day climatological observations. Attempts were made by Julia and Anthony Slingo to devise new radiation and cloud parameterization schemes that could calculate cloud amount from model variables, both in the 5- and 11-level GCMs.<sup>148</sup> On the modelling of sea-ice and oceans, initially, existing mixed-layer ocean and thermodynamic sea-ice models were adapted for use with GCMs. The development of more advanced sea-ice and ocean models followed, in cooperation with the Scott Polar Research Institute and the Department of Applied Mathematics and Theoretical Physics at Cambridge (and later the Physics Department at Oxford).<sup>149</sup>

Meanwhile, a research group led by John Mitchell expanded GCM studies of CO<sub>2</sub>-induced climate change. The Met.O.20's priority remained the development of techniques for assessing the seasonal and regional variations in the climate's response to increased CO<sub>2</sub>. Although there was a growing interest in these variations, many of the discussions

146 **A.J. Crane** 'Comments on Recent Doubts About the CO<sub>2</sub> Greenhouse Effect', *J. Appl. Meteorol.* **20** (1981): 1547-49; **S.B. Idso** 'Reply to A.J. Crane', *J. Appl. Meteorol.* **21** (1982): 748. The Met.O.20 scientists often mentioned the work of Newell, Dopplack and Idso in their technical reports, but did not themselves engage in the debate.

147 **British Met Office Annual Report, 1980-81**

148 Ibid. See also **J.M. Slingo** *Interactive Cloud and Radiation in the Meteorological Office General Circulation Models*, Met.O.20 Technical Note, No. II/157 (Bracknell: British Met Office, 1980); **A. Slingo** *Clouds and Radiation in the Meteorological Office 11-layer General Circulation Model*, Met.O.20 Technical Note, No. II/203 (Bracknell: British Met Office, 1983)

149 **British Met Office Annual Report, 1980-81.**

on the greenhouse effect were still centred on the mean changes in climate.<sup>150</sup> The Met.O.20 was unique in focusing on the seasonal and regional aspects of the greenhouse effect. From the perspective of the Met Office, it seemed rather obvious that the study of CO<sub>2</sub>-induced changes in variability, such as the frequency of extreme temperatures or rainfall in a given region, would be of greater social and economic importance than the study of changes in mean values.<sup>151</sup> Furthermore, the Met.O.20 continued to hold a view that GCMs should primarily be able to simulate the present atmosphere's dynamics and its variability as realistically as possible. These demanded not only the use of a high-resolution atmospheric GCM, but also the modelling of horizontal heat transport by the ocean, which could not be attained by using simple mixed-layer oceans. Hence, while by 1981 it was possible to couple a simple mixed-layer ocean model to the 5-level GCM,<sup>152</sup> the Met.O.20 adhered to the strategy of prescribing changes in sea surface temperatures until the completion of the dynamical ocean circulation model.

Mitchell and his co-workers repeated and analysed the CO<sub>2</sub> doubling experiments with the sea surface temperature increased by 2 °C everywhere, and the sea ice extent kept constant.<sup>153</sup> The warming estimate of 2.6–2.8 °C for doubled CO<sub>2</sub> was reproduced, close to the NAS Study Group's "most probable" value. What was more important to the Met.O.20, however, was that the model's response to increasing CO<sub>2</sub> concentrations varied considerably with season and location. The simulation results demonstrated that there were large temperature rises in high latitudes in winter. In summer, some of the largest increases in temperature occurred in dry regions, especially central South America,

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150 For instance, Hansen's 1981 paper mentioned above was based on the results from 1-D RCMs. Manabe's group at GFDL did carry out GCM experiments to investigate the seasonal and latitudinal variation of the CO<sub>2</sub>-induced changes of hydrological variables. As already described, however, the effects of horizontal heat transport by ocean currents were neglected in their study. See **Manabe and Stouffer** 'Sensitivity of a Global Climate Model to an Increase of CO<sub>2</sub> Concentration in the Atmosphere'; **S. Manabe et al.** 'Summer Dryness Due to an Increase of Atmospheric CO<sub>2</sub> Concentration', *Climatic Change* **3** (1981): 347-86.

151 **J.F.B. Mitchell** *Statistical Testing of Climate Change Experiments*, Met.O.20 Technical Note, No. II/161 (Bracknell: British Met Office, 1981); **British Met Office** 'Special Topic - Carbon Dioxide and Climate', in *Annual Report of the Meteorological Office, 1982* (London: HMSO, 1982): 75-89.

152 **J.F.B. Mitchell** *The Coupling of an Oceanic Mixed Layer Model to an Atmospheric General Circulation Model to Study Air-Sea Interaction*, Met.O.20 Technical Note, No. II/162 (Bracknell: British Met Office, 1981). Although it took some time before the Met.O.20 actually used a coupled ocean-atmospheric GCM in CO<sub>2</sub> experiments, the study of coupling continued at the branch. See, e.g., **C. Gordon and M. Bottomley** *A Scheme for Incorporating the Thermodynamic Sea-Ice Model into a Coupled Ocean-Atmosphere Model*, Dyn. Clim. Technical Note No. 1 (Bracknell: British Met Office, 1984); *The Parameterization of the Upper Ocean Mixed Layer in Coupled Ocean-Atmosphere Models*, Dyn. Clim. Technical Note No. 2 (Bracknell: British Met Office, 1984).

153 **Mitchell** *Statistical Testing of Climate Change Experiments*; **British Met Office** 'Carbon Dioxide and Climate'.



Southern Africa and the Sahara. The variations in precipitation were no less conspicuous. Precipitation decreased over many regions in the subtropics, but increased in large areas of the inner tropics in summer and over much of high latitudes, Australia and the eastern coast of South America in winter. In the following years, partly funded by the EEC Climatology Programme, a detailed analysis was performed on the effects of increasing CO<sub>2</sub> on European climate. The numerical experiment with the 5-level model showed that the CO<sub>2</sub> increase in the model atmosphere led to a marked winter warming in Northern Europe and a dry climate in Southern Europe throughout the year.<sup>154</sup>

A difficulty encountered in this type of GCM experiment was to detect local aspects of the model's response above the level of the model's natural variability. To distinguish this "signal" from the "noise", Mitchell earlier executed the integration for a ten-fold increase in CO<sub>2</sub> without changes in sea surface temperatures, and contrasted the results with those from the corresponding doubling simulation. He deduced from this work that the "signal" was well above the "noise" level, and that the model responded in an expected manner as CO<sub>2</sub> was increased.<sup>155</sup> The detection of the model's signal was, of course, by no means straightforward. In the EEC-funded research, Mitchell and others compared the CO<sub>2</sub> sensitivities of the Met.O.20 5- and 11-level GCMs over Europe. It turned out that, where the two models' control simulations differed seasonally and regionally, so did their perturbed simulations.<sup>156</sup> This suggested that the projected increase in CO<sub>2</sub> might indeed be regarded as a perturbation, but that the isolation of the "signal" would be difficult.

Another problem, specific for the Met.O.20, was the unrealistic assumption of a uniform increase in sea surface temperatures. In the observed climate, sea surface temperatures were reported to exhibit significant variations over the continents and through the year. New simulation experiments were therefore carried out, in which these variations were prescribed as a function of latitude.<sup>157</sup> In one experiment using this

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154 **J.F.B. Mitchell et al.** *An Evaluation of the Impact of an Increase in CO<sub>2</sub> in the European Climate - Final Report of CEC Contract CL1-030-81-UK(H)* (Bracknell: British Met Office, 1984); **C.A. Wilson and J.F.B. Mitchell** *The 5-Layer Climate over Western Europe and the Frequency of Occurrence of Extreme Values of Temperature, Precipitation and Wind for Selected Grid Boxes - The Changes with 4 x CO<sub>2</sub> and Prescribed Sea Surface Temperatures*, Met.O.20 Technical Note, No. II/224 (Bracknell: British Met Office, 1984)

155 **Mitchell** *Statistical Testing of Climate Change Experiments*; **British Met Office** 'Carbon Dioxide and Climate'.

156 The 11-level GCM had much finer resolution in high latitudes and different boundary layer and convection schemes. **J.F.B. Mitchell** *On Modelling the Effects of CO<sub>2</sub> on Climate*, Dyn. Clim. Technical Note, No. 13 (Bracknell: British Met Office, 1984)

157 On the basis of previous integrations, the ocean temperature was assumed to respond locally and linearly to

modified strategy, CO<sub>2</sub> amounts were quadrupled, and the sea surface temperature increments, ranging from 2 °C in the tropics to over 5 °C in high latitudes, were imposed.<sup>158</sup> Mitchell's group contended that the resulting changes in the hydrological cycle qualitatively agreed with those in the earlier experiment. In fact, a larger temperature rise was found in high latitudes, and the mean surface warming, inferred for doubled CO<sub>2</sub>, increased to *ca.* 4 °C. But this stronger response was not given full attention. The temperature change over most of the earth's surface was prescribed, and a larger warming was expected from the nature of prescribed changes. Furthermore, the Met.O.20 GCM simulation produced only the "equilibrium" response to instantaneously increased CO<sub>2</sub>. The "transient" experiment with accurate modelling of the ocean and of other more slowly varying components, it was believed, would introduce a lag into the surface warming.<sup>159</sup>

At the Met.O.13, a great deal of effort had been put into the development of long-range forecasting. The collection and processing of climatic data had been largely organized to suit this purpose. Gradually, the observational studies of global climate change were conducted more systematically. Upon the retirement of M.K. Miles in 1978, David Parker was assigned to take over his analysis of climate change in the Northern Hemisphere.<sup>160</sup> In the early 1980s, among other activities, Parker set out to build a reliable hemispheric data set for upper-air temperature since 1948.<sup>161</sup> The available data contained many errors and uncertainties, obscuring small changes in climate, and were subject to meticulous quality control. The problem of quality was also evident in the Met.O.13's global sea surface temperature data, compiled back to 1854 by P.G.F. Caton.<sup>162</sup> In late 1980, as Caton retired, Chris Folland assumed the responsibility for the sea surface temperature work.

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changes in the surface heat balance. **A. Gilchrist** 'Increased Carbon Dioxide Concentrations and Climate - The Equilibrium Response', in *Carbon Dioxide - Current Views and Developments in Energy/Climate Research*, W. Bach *et al.* (eds.) (Dordrecht: D. Reidel, 1983): 219-58; **J.F.B. Mitchell and G. Lupton** *A 4 x CO<sub>2</sub> Integration with Prescribed Changes in Sea Surface Temperature*, Met.O.20 Technical Note, No. II/198 (Bracknell: British Met Office, 1983)

158 **Mitchell and Lupton** *A 4 x CO<sub>2</sub> Integration with Prescribed Changes in Sea Surface Temperature*.

159 For one of the first transient experiments, see **K. Bryan *et al.*** 'Transient Climate Response to Increasing Atmospheric Carbon Dioxide', *Science* **215** (1982): 56-58.

160 **D.E. Parker** *Interview by the author* (October 5, 1999). Parker previously worked at the Met.O.20's tropical meteorology group. During the mid 1970s, he was involved in the Met.O.20's data processing for the GARP Atlantic Tropical Experiment.

161 **D.E. Parker** *Climatological Monthly Mean and Normal Upper-Air Station Data - Layout of the New Data Sets*, Met.O.13 Memorandum No. 96 (Bracknell: British Met Office, 1980); *Quality Control of Monthly Mean Upper-Air Station Data*, Met.O.13 Memorandum No. 97 (Bracknell: British Met Office, 1980); **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1980-83)

Together with Parker, he painstakingly updated and revised the database by adding recent data and applying corrections for changing methods of ship observation.<sup>163</sup> At the same time, a quality-controlled, global set of night-time marine air temperatures was created for the equivalent period. With more computing resources available, Parker and Folland could process more data during 1981-82 than the Met.O.13 had done in the previous fifteen years.<sup>164</sup>

By 1983-84, the Met Office Historical Sea Surface Temperature (MOHSST) and Historical Marine Air Temperature (MOHMAT) data sets were completed.<sup>165</sup> These data sets were in good agreement with the previously reported trend – a warming of *ca.* 0.6 °C between about 1910 and the 1940s, and a subsequent cooling of *ca.* 0.3 °C to around 1970. This long-term fluctuation was as large as the temperature rise that the equilibrium modelling studies predicted for the observed increase of atmospheric CO<sub>2</sub>, and was taken to indicate that factors other than CO<sub>2</sub> were operating to cause climate variations.<sup>166</sup> In the more recent data, a slight warming was noticeable, particularly in the Southern Hemisphere. In the US, Hansen's group at NASA GISS claimed that an analogous recent warming in surface air temperature was consistent with the simulated greenhouse effect.<sup>167</sup> The Met.O.13 scientists were more cautious, and concluded that a recent warming should not necessarily be interpreted as part of a longer, global trend. Parker went on to argue that, even if the observational analysis were limited to certain features of

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162 **Caton and Lawes** *Analysis of Historic Sea Surface Temperature Data*.

163 **Folland** *Interview by the author*, **C.K. Folland and F. Kates** *Changes in Decadally Averaged Sea Surface Temperature over the World 1861-1980*, Met.O.13 Memorandum No. 128 (Bracknell: British Met Office, 1983); **British Met Office Annual Report, 1980-83**. Folland began his Met Office career at the Instrumentation Branch. He worked briefly at the Met.O.13 in the mid 1970s, and was then promoted to the Agriculture and Hydrometeorology Branch. In 1980, he rejoined the Met.O.13.

164 **Folland** *Interview by the author*. By 1981, the Met Office's central computing system, known as COSMOS, was based on three linked powerful processors – the CDC Cyber 205, the IBM 360/195, and the smaller IBM 370/158. A year later, the IBM 360/195 was replaced by the IBM 3081, a new machine of similar power. **British Met Office Annual Report of the Meteorological Office** (London: HMSO, 1981-82)

165 **J.H. Minhinick and C.K. Folland** *The Meteorological Office Historical Sea Surface Temperature Dataset (MOHSST)*, Met.O.13 Memorandum No. 137 (Bracknell: British Met Office, 1984); **D.E. Parker** *The Meteorological Office Historical Marine Air Temperature dataset (MOHMAT)*, Met.O.13 Memorandum No. 138 (Bracknell: British Met Office, 1984); **C.K. Folland et al.** 'Worldwide Marine Temperature-Fluctuations 1856-1981', *Nature* **310** (1984): 670-73.

166 **British Met Office Annual Report of the Meteorological Office** (London: HMSO, 1984)

167 Hansen *et al.* admitted that other natural factors such as solar variability or volcanic dust might have played an important role in past climate change, but assumed that eventually the effects of CO<sub>2</sub> would become more important. **J. Hansen et al.** 'Climate Impact of Increasing Atmospheric Carbon Dioxide', *Science* **213** (1981): 957-66. Some US scientists felt that Hansen group's conclusion was unwarranted. For the critiques of Hansen *et al.*'s paper, see **S.I. Rasool** 'On Predicting Calamities', *Climatic Change* **5** (1983): 201-02; **M. MacCracken** 'Climatic Effects of Atmospheric Carbon Dioxide', *Science* **220** (1983): 873-74; **S.B. Idso** 'Climatic Effects of Atmospheric Carbon Dioxide', *Science* **220** (1983): 874.

CO<sub>2</sub>-induced climate change that were calculated to display a larger signal-to-noise ratio, a few more decades would be needed for detection of such changes because of the natural climate variability.<sup>168</sup>

There were thus reservations and scepticism about the immediacy of the CO<sub>2</sub> influence, both in the Met.O.13 and the Met.O.20. Nevertheless, the Met Office scientists' caution did not prevent them from progressively moving towards global warming research in the early to mid 1980s. It is interesting to note that this development was accompanied with changes within the Met Office itself.

In late 1983, John Houghton, professor of atmospheric physics at Oxford, was appointed to succeed John Mason as director-general.<sup>169</sup> Mason and Houghton were both from academia. As chairman of the Joint Scientific Committee for WCRP, Houghton, like Mason, was well aware of the importance of climate research. However, their styles of directorship were quite different. For Mason, the most urgent task was to establish the Met Office as the leading scientific institution through its advanced numerical modelling capability. He recruited many young scientists into the Office and energetically promoted research, but held an inward-looking view of the organization, preserving the hierarchical and closed culture of the British Scientific Civil Service.<sup>170</sup> Houghton, on the other hand, had an outward-looking attitude, and building on the achievements made under Mason, could bring a more open research environment into the Office. For example, he encouraged scientific officers to communicate with the wider scientific community. The Met.O.20 had produced more than two hundreds technical reports up until 1983. Only a few of them appeared in the scientific journals. John Mitchell's reports on the CO<sub>2</sub> work were not on the list. Since around 1983-84, the Office scientists published more widely.<sup>171</sup>

The group that benefited most from Houghton's appointment was probably the

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168 **D.E. Parker** *A Search for a Postulated Effect of Increasing Atmospheric Carbon Dioxide using Worldwide Radiosonde Data*, Met.O.13 Memorandum No. 134 (Bracknell: British Met Office, 1984)

169 Houghton had also been deputy director of Rutherford Appleton Laboratory since 1979 and chairman of the Joint Scientific Committee for WCRP since 1981. As has already been noted, later in 1988, he was appointed as chairman of the IPCC's scientific assessment working group. **J.T. Houghton** *Interview by the author* (October 4, 1999)

170 It should also be considered that the Met Office had been an agency of the Ministry of Defence (MoD). In 1990, the Met Office became an executive agency, and in 1996, was made a Trading Fund, beginning commercial contracts with its customers. However, since the Hadley Centre for Climate Research and Prediction was set up in 1990, much of the Office's climate research has been funded by the Department of Environment.

171 **Mitchell** *Interview by the author*.

Met.O.13. While Mason was critical of Lamb's work, he appreciated some of the work done by the Met.O.13 and did not remove all the resources for the branch.<sup>172</sup> Yet, under his directorship, there was a strong predilection at the Met Office for physico-mathematical approaches. The compilation and empirical analysis of past climatic data, whether they were for long-range forecasting or for the study of climate change, were at the fringe of the Office's activities. When in 1980 the Thatcher government introduced cutbacks in the public sector, the Met.O.13 was one of the Office's main targets for reduction in staff. The decision was made to stop the public issue of monthly and seasonal forecasts. The Met.O.13 itself was also under heavy pressure to close down.<sup>173</sup> Parker was nearly moved to the Central Forecasting Office, only to be saved by Gilchrist's strong appeal against it.<sup>174</sup> With Houghton as new director-general, the Met Office took a somewhat more accommodating approach towards a range of diverse research activities. Consequently, Parker and Folland could more securely carry out the observational studies of climate change.<sup>175</sup>

The change in directorship coincided with the increasing role in research design and implementation of the next generation of scientists who joined the Met Office in the late 1960s and early 1970s. Many of these scientists, including Mitchell, Julia and Anthony Slingo, Parker and Folland, were physicists by training and, besides one or two years of temporary posts, had not been involved in short-range weather forecasting. For the Met.O.13, this meant that climate data began to be processed more rigorously than in the previous practice of synoptic or historical climatology. At the Met.O.20, generational transition brought about changes in the style of climate modelling. As described above, the Met.O.20's version of physical realism, and the associated emphasis on the seasonal and regional variations, required a high-resolution atmospheric GCM. Due to computational limitations at the time, this approach could only allow short integrations with prescribed sea surface temperatures. Mitchell, however, wanted to perform perturbed climate change simulations (above all, enhanced CO<sub>2</sub> experiments), and was willing to

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172 **Folland** *Interview by the author*; **Parker** *Interview by the author*.

173 **British Met Office Annual Report, 1980-81**; **J.T. Houghton and P.W. White** 'Retirement of Mr A. Gilchrist', *Meteorol. Mag.* **118** (1989): 21-22.

174 **Parker** *Interview by the author*. This was before Folland rejoined the Met.O.13. Parker was the only scientist who was actively involved in the observational studies of climate change.

175 *Ibid.*; **Folland** *Interview by the author*. It might also have helped the Met.O.13 that the former head of the branch, D.M. Houghton, was John Houghton's brother.



use a relatively low-resolution, atmosphere-ocean coupled model for long integrations.<sup>176</sup>

Not all second-generation scientists at the Met.O.20 subscribed to Mitchell's perspective. For instance, Anthony Slingo, head of the branch's physical processes group, preferred a high-resolution GCM that could incorporate complex parameterizations, and was reluctant to trade off the model resolution for the length of integration.<sup>177</sup> Slingo's approach also differed from that of first-generation Met.O.20 scientists: his criterion for choosing parameterization schemes was a realistic representation of individual physical processes, not necessarily of atmospheric dynamics. The Met.O.20's style of modelling therefore did not simply change from one to another. As GCM developments matured, along with generational changes, the division of scientific labour emerged between those who used the model for climate change simulations, those who wanted to improve the physical basis of the model, and those who were more concerned with the dynamical aspects.<sup>178</sup> Nor did Houghton's assumption of directorship radically transform the hierarchical structure of the Met Office's research. Many of the judgments in climate research continued to be made at the senior level, sometimes at the level of deputy directorate of dynamical research, enabling the management and coordination of the co-existing heterogeneous styles in modelling.

### *Climatic Research Unit*

Although Parker and Folland had a broad interest in global climatic trends, they tended to focus on sea surface and marine air temperatures. Again, this reflected the institutional goals and interests of the Met Office. The Office had long considered it important to study the effects of ocean temperature anomalies on large-scale atmospheric circulation changes, which was expected to provide a scientific basis for medium- or long-range forecasting. It was for this reason that the Met.O.13 initiated a research programme on global sea surface temperatures in 1973.<sup>179</sup> Indeed, in 1984-85, Folland's analysis of MOHSST suggested that there might be a strong correlation between dry and wet periods in the African Sahel

<sup>176</sup> Mitchell *Interview by the author*.

<sup>177</sup> A. Slingo *Interview by the author* (October 8, 1999).

<sup>178</sup> Ibid.; Mitchell *Interview by the author*. Since 1982, the Forecasting Research Branch (Met.O.11) also began to use a global weather forecasting model, based on the design of the Met.O.20 GCM. Not surprisingly, scientists at the Met.O.11 tended to focus on the numerical and dynamical aspects of the model. M.J.P. Cullen *Interview by the author* (July 26, 2000). For the Met.O.11 global forecasting model in the early 1980s, see A.J. Gadd 'The 15-Level Weather Prediction Model', *Meteorol. Mag.* **114** (1985): 222-26.

<sup>179</sup> See Chapter 3.

and contrasting patterns of sea surface temperature anomalies on a near global scale.<sup>180</sup> These observations accorded with the results of GCM experiments conducted by the Met.O.13's dynamical long-range forecasting group.<sup>181</sup> Houghton was very impressed by these results, and became more supportive of the Met.O.13.<sup>182</sup>

For land-based surface temperatures, though, the Met.O.13's data work was still regionally oriented and for the most part confined to the improvement of the Central England Temperature series, going back to 1772. The initiative to develop a global/hemispheric data set was instead taken by Tom Wigley, Phil Jones, Mick Kelly and other members at CRU. By 1980-81, the Unit had completed a long-term series of monthly mean surface air temperatures for the period since 1881 over the Northern Hemisphere and the Arctic regions. The analysis of these data strengthened the earlier contention that the long-term cooling during the 1940s to 1960s had come to an end. Jones *et al.* also reported that in recent years there was a renewed warming trend, with 1980-81 being among the five warmest winters for the last 100 years, even though the commencing time of this warming varied from season to season – mid to late 1960s in winter and spring, mid 1970s in autumn and even later in summer.<sup>183</sup> The warming of the Arctic was not yet construed as a trend because of its insufficient duration, but the overall pattern of Arctic temperatures was found to be comparable to that for the Northern Hemisphere.<sup>184</sup>

By that time, there were other major publications on long-term records of land-based surface temperatures, most notably by Hansen *et al.* at NASA GISS and by Konstantin Ya. Vinnikov *et al.* in Russia.<sup>185</sup> Despite some differences in details, these records correlated

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180 **C.K. Folland *et al.*** *Sahel Drought and Worldwide Sea Surface Temperatures*, Met.O.13 Memorandum No. 160 (Bracknell: British Met Office, 1985); **C.K. Folland** *Speculations on Possible Causes of the Sahel Drought and Other Recent Large-scale Climatic Fluctuations*, Met.O.13 Memorandum No. 164 (Bracknell: British Met Office, 1985); **C.K. Folland *et al.*** 'Sahel Rainfall and Worldwide Sea Temperatures, 1901-85', *Nature* **320** (1986): 602-07.

181 **Folland *et al.*** *Sahel Drought and Worldwide Sea Surface Temperatures*; **T.N. Palmer and D.A. Mansfield** 'Response of 2 Atmospheric General Circulation Models to Sea Surface Temperature Anomalies in the Tropical East and West Pacific', *Nature* **310** (1984): 483-85; **T.N. Palmer and S. Zhaobo** 'A Modeling and Observational Study of the Relationship between Sea Surface Temperature in the Northwest Atlantic and the Atmospheric General Circulation', *Q. J. Roy. Meteor. Soc.* **111** (1985): 947-75.

182 **Folland** *Interview by the author*.

183 **Jones *et al.*** 'Variations in Surface Air Temperatures - Part 1.' In fact, the spring and winter of 1981 were the warmest on record.

184 **Kelly *et al.*** 'Variations in Surface Air Temperatures - Part 2.'

185 **Hansen *et al.*** 'Climate Impact of Increasing Atmospheric Carbon Dioxide'; **K.Y. Vinnikov *et al.*** 'Current Climatic Changes in the Northern Hemisphere', *Soviet Meteorology and Hydrology* **6** (1980): 5176.

highly with CRU data on annual and longer time scales. All three temperature series revealed a more pronounced warming in the Northern Hemisphere since around 1970 than in the Met.O.13's ocean temperatures. While these studies relied on the same basic data sources, they used quite different methods of producing area averages, increasing confidence in the results. Hansen and his collaborators, in particular, were intrigued by the recent warming trend. As noted earlier, they argued that this could be the result of increase in atmospheric CO<sub>2</sub>. The Hansen group went as far as to speculate that "anthropogenic carbon dioxide warming should emerge from the noise level of natural climate variability by the end of the century, and there is a high probability of warming in the 1980's."<sup>186</sup> Yet CRU scientists did not share the Hansen group's conclusion. Like Parker and Folland, they took a cautious stance, claiming that there was "as yet, no statistical basis for associating this recent warming with the effects of increasing atmospheric carbon dioxide"<sup>187</sup>

The work of the Hansen group on long-term temperature data was organized specifically to assist their modelling studies into CO<sub>2</sub>-induced global warming.<sup>188</sup> By contrast, the Unit's interest in historical climatic records began with its inception in the early 1970s, and could be traced back to Hubert Lamb's earlier work in the 1950s and 1960s. The observational studies by CRU were thus intended for a more general understanding of past climate changes.<sup>189</sup> However, unlike at the Met.O.13, the question of global warming surfaced more naturally in CRU's research. The construction of global/hemispheric land-based temperature data sets was funded in part by USDOE's Carbon Dioxide Research Division,<sup>190</sup> and though not always explicitly stated, the issue of CO<sub>2</sub>-induced climate change was always in the background of this work. USDOE funding was also important in that it gave the Unit more opportunity to directly communicate with US scientists engaged in global warming research. For instance, since the USDOE-AAAS workshop in early 1979, the Unit's director, Tom Wigley, regularly attended the meetings sponsored by USDOE's CO<sub>2</sub> research programme.

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186 Hansen *et al.* 'Climate Impact of Increasing Atmospheric Carbon Dioxide'.

187 Jones *et al.* 'Variations in Surface Air Temperatures - Part 1.'

188 The occurrence in 1980 of a very hot and dry summer in the southern US might also have helped to draw more attention to the recent warming trend in the US. See, *e.g.*, J.A. Wagner 'Worse Heat-wave in 26 Years Grips South Central United States', *Weatherwise* 33 (1980): 168-69.

189 Jones *Interview by the author*, Kelly *Interview by the author*.

CRU scientists by no means responded passively to the research interests of the funding agency. As briefly mentioned in the previous chapter, from as early as the late 1970s, Wigley recognized that CO<sub>2</sub>-induced global warming was becoming an important research agenda and pushed CRU into that direction. Other members of the younger generation generally supported Wigley's move. Their position on the issue of global warming in the early to mid 1980s was perhaps best summarized in the following comments published in 1981:

It is difficult to know the significance of these events [*i.e.* recent winter warmth and less snow cover], but it is certainly premature to associate them with the effects of increasing atmospheric carbon dioxide. ... However, a few more consecutive warm winters, with continuing warmth in the other seasons, will doubtless give climatologists pause for thought.<sup>191</sup>

So it was seen as essential to keep improving and expanding past temperature records and updating data sets with the latest observations. At first, Antarctic temperatures were added, which indicated a significant warming of *ca.* 0.7 °C since 1960 in the annual mean temperature series.<sup>192</sup> By 1985, in collaboration with Bradley and Diaz in the US, CRU scientists revised their data sets of monthly mean land-based temperatures to include both the Northern and Southern Hemispheres for the period 1851-1984.<sup>193</sup> Similar strong warming trends were found in all land-based series since 1965. The analysis was subsequently extended to the first “global” synthesis of near surface temperatures over the land and ocean.<sup>194</sup> There were some discrepancies between data sets,<sup>195</sup> but annual global mean temperatures gave a rather clear warming trend, with the three warmest years being 1980-81 and 1983, and five of the nine warmest years occurring after 1978. As it soon turned out, CRU's temperature records did not show some of the characteristics predicted

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190 The US Office of Naval Research also supported the early part of CRU's observational studies, but USDOE remained as a major funding source throughout the 1980s.

191 T.M.L. Wigley *et al.* 'Global Warming?' *Nature* **291** (1981): 285.

192 S.C.B. Raper *et al.* 'Recent Temperature Changes in the Arctic and Antarctic', *Nature* **306** (1983): 458-59; Raper *et al.* 'Variations in Surface Air Temperatures: Part 3. The Antarctic, 1957- 82'.

193 P.D. Jones *et al.* 'Northern Hemisphere Surface Air Temperature Variations: 1851- 1984', *J. Clim. Appl. Meteorol.* **25** (1986): 161-79; 'Southern Hemisphere Surface Air Temperature Variations: 1851- 1984', *J. Clim. Appl. Meteorol.* **25** (1986): 1213-30.

194 P.D. Jones *et al.* 'Global Temperature Variations Between 1861 and 1984', *Nature* **322** (1986): 430-34.

195 In the Northern Hemisphere, the marine data showed a slightly later onset of the recent warming. In the Southern Hemisphere, however, the corresponding temperature rise was more evident over the ocean.

for CO<sub>2</sub>-induced global warming by modelling studies.<sup>196</sup> They nevertheless played a critical role in further disseminating the global warming hypothesis.

In addition to the compilation and analysis of temperature records, again funded by USDOE, CRU pursued several other research projects that were more directly targeted at the assessment of CO<sub>2</sub>-induced climate changes and their impacts. Scientists at the Unit believed that, due to the deficiencies of current numerical models, it would be unwise to depend totally upon the model results.<sup>197</sup> The projects were therefore designed in such a way as to capitalize on CRU's expertise and skills in observational studies. For example, an alternative, empirical method was developed to derive a scenario for CO<sub>2</sub>-induced climate changes. Spatial and seasonal patterns of temperature, precipitation and sea-level pressure were extracted from individual warm years in a given period of the early to mid 20<sup>th</sup> century, and were used as analogues for the future warm, high-CO<sub>2</sub> world.<sup>198</sup> The resulting climate scenarios were then used to construct scenarios of the impact of a global warming on energy consumption and agriculture.<sup>199</sup> By comparing observed temperature changes between warm and cool years with changes computed to result from increasing atmospheric CO<sub>2</sub>, CRU scientists also examined the signal-noise ratio, giving insight into the possible detection of CO<sub>2</sub>-induced climate changes.<sup>200</sup>

During the course of its research activities, the Unit used the Met Office archive extensively. As Lamb, and later Mason, retired, the early tension between the two institutions abated.<sup>201</sup> Houghton's more open approach also paved the way for collaboration. And yet, they did not develop a close relationship, partly because the Met.O.13 was not as interested in the issue of global warming as CRU was. Hence, when the Unit combined land and ocean temperatures to develop global land-based data,

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196 **P.D. Jones** 'Hemispheric Surface Air Temperature Variations - Recent Trends and an Update to 1987', *J. Climate* **1** (1988): 654-60.

197 **P.M. Kelly et al.** 'Comment on "Detecting Carbon Dioxide Effects on Climate" by W.H. Klein', in *Carbon Dioxide Review 1982*, W.C. Clark (ed.) (Oxford: Oxford University Press, 1982): 246-51.

198 **Wigley** 'The Impact of Future Atmospheric CO<sub>2</sub> Increases'; **Wigley et al.** 'Scenario for a Warm, High-CO<sub>2</sub> World'; **A.B. Pittock and M.J. Salinger** 'Towards Regional Scenarios for a CO<sub>2</sub>-Warmed Earth', *Climatic Change* **4** (1982): 23-40.

199 **J.M. Lough et al.** 'Climate and Climate Impact Scenarios for Europe in a Warmer World', *J. Clim. Appl. Meteorol.* **22** (1983): 1673-84.

200 **T.M.L. Wigley and P.D. Jones** 'Detecting CO<sub>2</sub>-Induced Climatic Change', *Nature* **292** (1981): 205-08; *Signal-to-Noise Ratios for Surface Air Temperature and the Detection of CO<sub>2</sub>-induced Climatic Change*, Proceedings of the Workshop on First Detection of Carbon Dioxide Effects, DOE/CONF-8106214 (Washington, DC: US Department of Energy, 1982)

201 **Folland** Interview by the author.



NOAA's Comprehensive Ocean-Atmosphere Data Set (COADS) was used rather than MOHSST and MOHMAT. Within CRU, Lamb remained sceptical about the global warming hypothesis, and was critical of what he saw as "the bandwagon situation" that "could all too easily divert unreasonable sums into carbon dioxide research".<sup>202</sup> But having retired from active research, Lamb had little influence on the way the Unit's research was organized. Not only was his version of historical climatology replaced by a more quantitatively oriented version, but also interest in the climatic effects of volcanic eruptions and other natural agents, which he wanted to develop, was superseded by that of the CO<sub>2</sub> issue.<sup>203</sup>

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202 **H.H. Lamb** 'Book Review - *Future Weather*', *Interdiscipl. Sci. Rev.* **9** (1984): 365.

203 However, the issue of global warming did not entirely dominate the Unit's research. A diverse range of research topics were pursued. On volcanic effects on climate, P.M. Kelly continued to do some work during the early 1980s. See **C.B. Sear and P.M. Kelly** 'Eruptions of Mt. St. Helens - Effects on Climate', *Nature* **285** (1980): 533-35; **P.M. Kelly and C.B. Sear** *The Formulation of Lamb's Dust Veil Index*, Mount St. Helens Eruptions of 1980 - Atmospheric Effects and Potential Climatic Impact (Washington, DC: US GPO, 1982); 'Climatic Impact of Explosive Volcanic Eruptions', *Nature* **311** (1984): 740-43.

## Chapter 6. Disunity in the Science of Global Warming

### Diverse Subcultures and Hierarchy in Climate Science

In the previous chapters, I have attempted to trace, albeit sketchily, how modern climate science evolved through the period up to the mid 1980s, mainly in Britain and with respect to the issue of global warming. Before and for some time after World War II, studies of climate focused on collecting meteorological records and using them to describe and classify regional climates. As the notion of “climate normals” implied, climate, viewed as the synthesis of weather conditions over a particular region, was believed to vary within its average state but was assumed to be approximately constant for a period of several decades or centuries. Discussion of climate change was restricted to geological time-scales and fell into the realm of geologists, astrophysicists or palaeontologists. By the early to mid 1980s, scientists working on climate-related issues had a quite different identity that was more often associated with physico-mathematical modelling of global climate system. The research agenda also broadened to include short-term climate variations as well as climate changes for historical time-scales. The current form of global warming science builds upon this new strand of research on climate and its change.

The transition was by no means unidirectional from one type of research activity to another as a result of the inevitable, linear progress in science. Even in established sub-disciplines of physics, there are multiple, relatively autonomous scientific subcultures with distinct conceptual and methodological commitments and institutional settings.<sup>1</sup> Matters could hardly be less complicated in the field of climate science. Various sub-branches of science had been involved in or relevant to the studies of climate and its change. Consequently, the historical path of climate science was more complicated, straddling diverse scientific subcultures.

At least three lines of research played an important role in developing studies of how

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<sup>1</sup> See, for example, **P.L. Galison** *How Experiments End* (Chicago: University of Chicago Press, 1987); *Image and Logic - A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997)

climate had changed over non-geological time-scales.<sup>2</sup> First, historical climatology based on the reconstruction of past and more recent weather conditions to investigate climate variations and changes; secondly, dynamical climatology armed with complex physico-mathematical models incorporating the hydrodynamic and thermodynamic equations of global atmospheric circulation; and thirdly, a group of theoretical and semi-empirical studies of atmospheric radiation as a controlling factor for the earth's climate. As I have described, the first two research streams were more noticeable in Britain. Both traditions were born out of the Met Office's research activities, led by Hubert Lamb (historical climatology) and scientists at the Met.O.20 (dynamical climatology). While their embryonic origins might be found in the pre-World War II period, they took shape and became more visible from the late 1950s onwards.

It would be interesting at this point to mention the concepts of "trading zone" and "boundary objects". In his historical study of 20<sup>th</sup>-century microphysics, Galison shows that scientists and engineers at the boundaries between different subcultures were able to develop pidgin- or creole-like local interlanguages without necessarily attributing global meanings to them, thereby creating a "trading zone" that would allow practical "coordination of action and belief".<sup>3</sup> Similarly but somewhat differently, in their case study of Berkeley's Museum of Vertebrate Zoology, Star and Griesemer observe that different groups of actors were able to establish a "mutual *modus operandi*" by constructing "boundary objects" that would be "plastic enough to adapt to local needs and the constraints of the several parties employing them, yet robust enough to maintain a common identity across sites".<sup>4</sup> In these ways, the authors conclude, successful scientific cooperation can be achieved in the midst of disunity in science. Between Lamb's historical climatology and the Met.O.20's dynamical climatology, no such intermediary translation processes seem to have existed.

Lamb had originally been trained in geography before joining the Met Office; he rebelled against the great pressure upon him to study mathematics by his father, a professor of mechanical engineering, and the reputation of his grandfather Horace Lamb,

<sup>2</sup> This does not mean that other research streams were not important.

<sup>3</sup> Galison *Image and Logic*, at 803.

<sup>4</sup> S.L. Star and J.R. Griesemer 'Institutional Ecology, "Translations" and Boundary Objects - Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907-39', *Soc. Stud. Sci.* **19** (1989): 387-420, at 388, 393.

a famous hydrodynamicist.<sup>5</sup> Lamb's approach was descriptive, qualitative and interdisciplinary. He frequently published in geographical journals. And yet, as the subtitle of his autobiography "A Meteorologist's Tale" indicates, Lamb regarded himself as a professional meteorologist.<sup>6</sup> He was uncomfortable about what he saw as the arrogance of theoreticians, but this did not prevent him from sharing the basic concepts of meteorology. His own definition of climatology, which resembled that of dynamical meteorologists, illustrates this:

Climatology, the study of climate, the long-term aspects and total effects of meteorological processes, is (like meteorology) a branch of Earth-physics (geophysics). It is concerned with the various conditions of the atmosphere that occur and with everything that, habitually or occasionally, influences the condition of the atmosphere, either locally or over great regions of the Earth. Like any other branch of physics, it is also concerned with measuring the effects of such influences, and seeking to discover laws or principles of general application governing their behaviour and interconnexions.<sup>7</sup>

He recognized the benefits of physico-mathematical modelling in the studies of climate and its change, and repeatedly stressed this point in his articles and books.<sup>8</sup> To him, empirical studies of past and more recent climates were complementary to, not in conflict with, modelling studies. For they would "ensure that the performance of the theoretical models is related to fact and that indications are realistic as regards the range of variation which they predict."<sup>9</sup>

Lamb's argument for using historical climatology to validate model simulations might have promoted the development of trading languages or boundary objects that could mediate between the two research traditions. The dialogue between historical/palaeo-climatologists and dynamical meteorologists had not been entirely absent during the 1960s.<sup>10</sup> In the US, the Panel on Climatic Variation of the US Committee for GARP

5 Lamb later recalled, "[M]y father told me that I had 'only obtained a mediocre, mixed degree that I would regret all my life'. **H.H. Lamb** *Through All the Changing Scenes of Life - A Meteorologist's Tale* (East Harling: Taverner, 1997), at 31.

6 Ibid.

7 **H.H. Lamb** *Climate: Present, Past and Future - Vol. 1 Fundamentals and Climate Now* (London: Methuen, 1972), at 5.

8 See, e.g., **H.H. Lamb** *Climate, History and the Modern World*, 1<sup>st</sup> edition (London: Methuen, 1982)

9 **H.H. Lamb** 'Climate Engineering Schemes to Meet a Climatic Emergency', *Earth-Sci. Rev.* 7 (1971): 87-95.

10 See, e.g., **J.M. Mitchell** (ed.) *Causes of Climatic Change - A Collection of Papers Derived from the INQUA-*

(USC-GARP) included an observational climatologist J. Murray Mitchell, and its 1975 report *Understanding Climatic Change* gave significant space to the observational studies of past climate.<sup>11</sup> Such interactions did not take place in Britain. The problem was that the Met.O.20 scientists could find neither practical incentives nor scientific underpinning for them to collaborate with historical climatologists, meaning not just with Lamb but also those at the Met.O.13.

The Met Office's foremost research priority was the improvement of weather forecasting models. The Met.O.20's research had to fit into the available computing time, and its general circulation model (GCM) group had to spend this time just developing the models. Furthermore, as a branch of the national meteorological service, the Met.O.20 was initially more interested in the prospect of dynamical long-range weather forecasting. The temporal range that the Met.O.20 scientists had in mind was therefore much shorter than that of historical climatology. By the early to mid 1970s, meteorologists came to widely accept that the theoretical limit of long-range forecasting was no more than a few weeks. This coincided with the rising interest in climate change issues. In the meantime, the installation of new computers provided access to more computing time. With these backgrounds, and John Mason's backing, the Met.O.20 began to adopt a broader approach to climate modelling. But even then, as has been noted, the Met.O.20 held a view that GCMs should first be able to simulate the detailed short-term variability of the *present* climate. Research into past climates or longer-term climate changes was given second priority.

The fairly closed and hierarchical culture of the Met Office also discouraged research that deviated from the direction set by the higher level. In Lamb's case, he could ensure continuation of his historical climatology research at the Office chiefly because he was a special merit scientific officer, a post that had sufficient autonomy to undertake independent research.<sup>12</sup> At a more fundamental level, however, the issue at stake was how

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NCAR Symposium, Meteorological Monographs, Vol. 8, No. 30 (Boston: American Meteorological Society, 1968); **A.E.M. Nairn** (ed.) *Problems in Palaeoclimatology - Proceedings of the NATO Palaeoclimates Conference* (New York: Interscience Publishers, 1965); **J.S. Sawyer** (ed.) *World Climate from 8000 to 0 BC - Proceedings of the International Symposium* (London: Royal Meteorological Society, 1966)

11 USC-GARP 'Appendix A - Survey of Past Climates', in *Understanding Climatic Change - A Program for Action* (Washington, DC: National Academy of Sciences, 1975): 127-95.

12 Lamb was awarded a special merit promotion in 1963, when the Climatological Research Branch was divided into the Synoptic Climatology (Met.O. 13) and Dynamical Climatology (Met.O.20) Branches. While Lamb was attached to the Met.O.13, he was allowed more freedom to develop his own research. See **Lamb Through All**



the scientific study of climate and its change should be conducted. The Met.O.20's dynamical climatology, with its efforts to build GCMs, was a legitimate and respectable research programme of dynamical meteorology, which itself was considered as the most highly developed branch of an umbrella discipline. By contrast, Lamb's approach was perceived by mainstream meteorologists as violating their norms of scientific practice and method. Lamb also departed from previous climatological practices in the service tradition in that he applied some of the meteorological concepts such as atmospheric circulation to the studies of climate. Yet, his empirically based, interdisciplinary approach did not reflect the increasing application of physico-mathematical approaches in meteorology. Nor did it use advanced statistical techniques in data analysis. At a time of rapid mathematization and computerization in meteorology, Lamb's work was deemed to be too qualitative. To many, he was merely a geographer rather than a meteorologist.<sup>13</sup>

When Lamb advocated the need to study past climates and long-term climate changes, he met, quite naturally, with strong scepticism from those supporting dynamical climatology. As discussed in Chapter 3, director-general of the Met Office John Mason was very critical of Lamb's "intuitive and qualitative" way of doing science. Mainstream meteorologists' distrust persisted after Lamb left the Office to set up the Climatic Research Unit (CRU). In 1978, Reginald Sutcliffe, former director of dynamical research at the Office and professor of meteorology at Reading, reviewed volume II of Lamb's book *Climate, Present, Past and Future*. His assessment of Lamb's empirically based approach was harsh:

On the realistic human time scale one may also believe that the mathematical geophysicists, the meteorologists and oceanographers, will in due course make a more definite contribution than this book would lead one to expect and may well demonstrate what seems self-evident to some that the examples of so-called 'scientifically based forecasts' printed in the last appendix are for the most part scientifically worthless.<sup>14</sup>

These comments upset Lamb, and unusually for book reviews in *Quarterly Journal of the*

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*the Changing Scenes of Life* .

13 **R.S. Harwood** Interview by the author (February 25, 1999)

14 **R.C. Sutcliffe** 'Book Review - *Climate, Present, Past, and Future, Volume II: Climatic History and the Future*', *Q. J. Roy. Meteor. Soc.* **104** (1978): 536.

*Royal Meteorological Society*, he decided to reply:

There are some doubtless unconsciously extreme statements and implications in Professor Sutcliffe's review of my book ... Professor Sutcliffe's statement amounts to a claim that advice on current and future tendencies should be the sole preserve of the theoretician, based upon intensive (theoretical) study; he does not need to look at the past record of climate and perhaps even should not look at it. ... Can science expect to proceed usefully while ignoring a whole range of the observable phenomena that it has to interpret? And did it ever do so?<sup>15</sup>

Ten years on from this exchange, Lamb's idea of coexistence and collaboration between dynamical and historical climatology, once seen as awkward by mainstream meteorologists, was largely a reality. The cultural hierarchy of meteorology was not seriously questioned, and GCMs retained their pre-eminent position in climate science. Observational scientists working on historical climatology continued to feel that modellers did not fully appreciate their work.<sup>16</sup> Nonetheless, the model validation and the detection of "signal" using climate records turned into an integral part of the science of global climate change, involving both climate modellers and historical climatologists. As in Star and Griesemer's Berkeley Zoology Museum or in Galison's MIT Radiation Laboratory, observational scientists at CRU (which Lamb had founded) and at the Met.O.13 were collaborating with GCM scientists at the Met.O.20, despite their differences.

For this to happen, however, some kind of a common goal or set of incentives, no matter how ambiguous they might be, had first to be created or found before the collaborative processes could be developed to construct trading languages or boundary objects. In Star and Griesemer's case study, for example, all the participant groups shared a goal of conserving California's natural environment.<sup>17</sup> Lamb and other historical climatologists acknowledged the value of GCMs although their assessments of the reliability of the modelling approach varied. The Met.O.20 scientists as yet had to be drawn into the studies of past climates. The topic of "climate change" was too broad to serve this role. Ironically for Lamb, it was the issue of CO<sub>2</sub>-induced global warming that made the Met.O.20 scientists turn to historical climatology. While Lamb had been

15 H.H. Lamb 'Letter to the Editor', *Q. J. Roy. Meteor. Soc.* **104** (1978): 1008-09.

16 P.D. Jones *Interview by the author* (August 15, 2000)

17 Star and Griesemer 'Institutional Ecology, 'Translations' and Boundary Objects'.

sceptical of the global warming hypothesis all along, he was one of the first Met Office scientists to address its importance, and above all, the need for historical climatic data to study the problem.<sup>18</sup>

But the heightened interest in past climates alone was not enough. Historical climatology also had to be transformed. Upon Lamb's retirement from CRU, the second generation of observational scientists, such as Tom Wigley, Mick Kelly and Phil Jones, created a new historical climatology, shifting from qualitative descriptions to quantitative data analysis. By using statistical and other mathematical techniques, the information that Lamb literally wrote down in the synoptic weather charts was translated into multiple sets of quantitative parameters. The compiled data were then interpreted in the form and language that were more in tune with those of dynamical meteorologists or atmospheric scientists.<sup>19</sup> About the same time, the Met.O.13 accelerated a move towards more quantitative studies of global climate trends.<sup>20</sup> These changes, which seem comparable to the introduction of "standardized forms" in Star and Griesemer's study, were not imposed, but were hardly neutral. The metaphors of "trading zone", and to a lesser extent "boundary objects", imply that actors could participate in the negotiation on a more or less equal footing, and that their different understandings of a shared topic would be more or less equally respected and preserved. The interactions between different subcultures in the development of climate science were different. Unlike in Star and Griesemer's Museum and Galison's Radiation Laboratory, they were strongly constrained by the cultural hierarchy of science.

## Hierarchy within Climate Modelling

The hierarchical relation between different scientific subcultures was also evident in the way in which the third research stream mentioned earlier – that of theoretical and semi-

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18 See, e.g., **H.H. Lamb** 'Climatic Variation and Our Environment Today and in the Coming Years', *Weather* **25** (1970): 447-55.

19 It was significant that Wigley and younger scientific staff at CRU had different educational backgrounds from Lamb. See Chapter 5.

20 In fact, already in the mid 1960s, the Met.O.13 started using computer and advanced statistical techniques in processing some of the climatic records. See, e.g., **J.M. Craddock** 'The Analysis of Meteorological Time Series for use in Forecasting', *J. Roy. Stat. Soc. D-Stat.* **15** (1965): 167-90; 'Programming Systems for Use in Research Computation', *Comput. J.* **8** (1966): 315-18. However, until the late 1970s, its scope had been largely limited to the development of long-range forecasting, rather than studies of global climate and its change.

empirical studies of atmospheric radiation – was integrated into the evolution of GCM-centred climate science.

Atmospheric radiation had long been an important research area in the disciplines of meteorology and physics. Some of the studies on this subject were directly concerned with the role of the radiation budget in controlling the earth's climate.<sup>21</sup> It was the subsequent construction of simple climate models based on these early studies – radiative-convective models (RCMs), other types of radiative transfer models or energy balance models (EBMs) – that first pioneered physical research into climate processes and feedback, and the sensitivity of the earth's climate to natural and anthropogenic external forcings. The two US-led workshops in 1970-71, the Study of Critical Environmental Problems (CEP) and the Study of Man's Impact on Climate (SMIC), were a good example of this development. In the workshops, which were key events in the beginning of a new era of climate modelling, the panel discussions relied mostly upon the results from simple, non-dynamical models.<sup>22</sup> Since around that time, more variants of simple climate models of this kind had been developed and used extensively in the science of global climate and its change.

Already at SCEP and SMIC, however, the prevailing view was that the future of climate science would lie in the expansion of GCM developments. Large-scale 3-D numerical models, with the explicit treatment of atmospheric dynamics and its intricate interaction with other processes, appeared to be more attractive than 0-, 1- or 2-D models. This was especially true for dynamical meteorologists, who held a dominant position within the broad church of atmospheric sciences. The acceptance of GCMs as a major methodology did not lead to the exclusion of other modelling techniques. Simple model studies of specific physical processes (*e.g.*, radiative transfer) were highly regarded. Physico-chemical, and in certain cases mathematical, knowledge and skills used in the studies were well respected. GCM research groups themselves routinely carried out such studies in the course of formulating and testing parameterization schemes for their 3-D dynamical

21 For a review of these early studies, see **G.E. Hunt et al.** 'A History of Presatellite Investigations of the Earth's Radiation Budget', *Rev. Geophys.* **24** (1986): 351-56.

22 **SCEP** *Man's Impact on the Global Environment - Report of the Study of Critical Environmental Problems* (Cambridge: MIT Press, 1970); **SMIC** *Inadvertent Climate Modification - Report of the Study of Man's Impact on Climate* (Cambridge: MIT Press, 1971). Manabe had GCM results for CO<sub>2</sub> doubling, but did not present them at the SMIC. **S. Manabe** *Interview by P.N. Edwards* (AMS Tape Recorded Interview Project: March 14, 1998); **S.H. Schneider** *Interview by the author* (January 23, 2001)

models. Yet GCM scientists, and those who endorsed their approach, had doubts as to whether models such as RCMs or EBMs could be used as independent tools for studying climate and its change. These simple models were understood either as a didactic tool or as part of GCM parameterizations.

Over the next decade, as the issue of climate change received more publicity, the international atmospheric sciences community was increasingly drawn into climate research. There were as yet only a handful of GCM research groups in the world. GCMs nevertheless quickly gained epistemic authority in climate science, and in the cultural hierarchy centred around GCMs simple models were relegated to a peripheral role. In a recent review on the technical history of climate modelling, McGuffie and Henderson-Sellers sum up the atmosphere of the time:

Although single-minded individuals persevered with the development of simpler models, by the early 1980s, this diverse range of climate models seemed to be in danger of being overshadowed by one type: the atmospheric GCM. Considerable funding, and almost all the computational power used by climate modellers, was being consumed by atmospheric GCMs, and an ethos of 'big is beautiful' was evident.<sup>23</sup>

Such a tendency was not confined to any one country, but was particularly strong in Britain. The Met.O.20 used single column models, derived from its 5- and 11-level GCMs, only occasionally for checking new parameterization schemes or investigating unexplained changes in GCM experiments. As described in Chapter 4, the main aim of the Met.O.20's CO<sub>2</sub>-doubled RCM experiment in 1978 was to develop a new, more detailed convection parameterization scheme for GCMs. For the Met.O.20, the use of GCMs was the only proper way of doing climate science. Andrew Gilchrist, deputy director of dynamical research, argued:

[C]ontrolled experiments cannot be carried out on the atmosphere, at least not on anything approaching a global scale. In these circumstances, large numerical models provide the only feasible method of testing hypotheses. Without them research in climate can easily become (and indeed for a long time was) a series of uncheckable

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<sup>23</sup> K. McGuffie and A. Henderson-Sellers 'Forty Years of Numerical Climate Modelling', *Int. J. Climatol.* **21** (2001): 1067-109, at 1077.



speculations.<sup>24</sup>

Given the status of the Met Office in British meteorological research, this meant that most of the resources available for climate research were directed to GCMs. John Green at Imperial College complained that the Met Office “spends about as much running their model as goes into university education of meteorologists in Britain.”<sup>25</sup> But in universities, too, many academic meteorologists agreed with Gilchrist that large-scale numerical models would be the way forward. Major university meteorology and related departments took part in a consortium of the UK Universities’ Atmospheric Modelling Group. Although more theoretically oriented and on a smaller scale than the Met.O.20’s work, they made substantial efforts to contribute to general circulation modelling.

Simple climate models required much fewer resources than GCMs, and it was still possible for university researchers to use them if they wanted to. Simpler forms of dynamical models were, indeed, often employed. A research group led by D.R. Davies at Exeter conducted theoretical studies into the dynamical-physical interactions of global circulations using a 2-level quasi-geostrophic model.<sup>26</sup> Green’s 2-D statistical-dynamical model (SDM) approach also fell into this category.<sup>27</sup> Some of these academic meteorologists, notably John Green, used simple models not because they had limited access to computing time, which they did, but because they had a different conception of computer modelling. In their view, the realistic reproduction of atmospheric behaviour using large complex models would not automatically lead to a more reliable understanding. Green bluntly stated:

Earlier versions (of GCMs) demanded some judicious manipulation of parameters in order to reproduce the present climate. That this is said not to be true of later models may indicate greater skills in anticipating the required numbers or greater understanding of the relevant physical processes. How can we tell?<sup>28</sup>

24 A. Gilchrist 'Numerical Simulation of Climate and Climatic Change', *Nature* **276** (1978): 342-45, at 343.

25 G.J. Shutts and J.S.A. Green 'Mechanisms and Models of Climatic Change', *Nature* **276** (1978): 339-42, at 341.

26 H.B. Gordon and D.R. Davies 'Mathematical Prediction of Climatic Change', *Nature* **253** (1975): 419-20; 'Sensitivity of Response of Climatic Characteristics in a 2- Level General Circulation Model to Small Changes in Solar Radiation', *Tellus* **29** (1977): 484-501.

27 J.S.A. Green 'Transfer Properties of the Large-scale Eddies and the General Circulation of the Atmosphere', *Q. J. Roy. Meteor. Soc.* **96** (1970): 157-85.

28 Shutts and Green 'Mechanisms and Models of Climatic Change', quote at 341.

They preferred instead something similar to what Dahan calls “laboratory models”.<sup>29</sup> The model physics and dynamics were kept reasonably simple so that simulation results could be more effectively diagnosed by isolating the structures at issue.<sup>30</sup>

This type of modelling approach was familiar to meteorologists and atmospheric physicists. As far as British climate modelling was concerned, it was pursued by only a few research groups, and with an exclusive emphasis on the theoretical-dynamical aspects of large-scale atmospheric motions. What was more peculiar was that there was even less interest in RCMs and EBMs that primarily modelled the thermodynamics of climate, and could be readily applicable to climate sensitivity/change studies. On the one hand, most British meteorologists and atmospheric physicists thought that EBMs were too simplistic and intellectually less interesting.<sup>31</sup> Even Green, a GCM sceptic, claimed that these models were rudimentary, lying “somewhere between physics and geography”.<sup>32</sup> EBMs rather tended to be discussed by physical geography groups whose interest was the effects of albedo changes on climate.<sup>33</sup> On the other hand, those scientists specializing in atmospheric radiation, and thus equipped to develop and use RCMs, were indifferent to the question of global climate and its change. For instance, Oxford’s atmospheric physics group, which had devised the first radiative transfer scheme for the Met.O.20 GCM, moved away from climate modelling just when climate research was growing in popularity internationally.<sup>34</sup>

British reluctance to exploit RCMs and EBMs contrasted with the US situation, where simple climate models of various kinds were widely used both in universities and in

29 **A. Dahan Dalmedico** ‘History and Epistemology of Models - Meteorology (1946-1963) as a Case Study’, *Arch. Hist. Exact Sci.* **55** (2001): 395-422.

30 **J.S.A. Green** *Interview by the author* (August 3, 1999)

31 One exception was the work of Drazin at Bristol. See **P.G. Drazin and D.H. Griffel** ‘Branching Structure of Diffusive Climatological Models’, *J. Atmos. Sci.* **34** (1977): 1696-706; **G.R. North et al.** ‘Energy Balance Climate Models’, *Rev. Geophys.* **19** (1981): 91-121.

32 **Green** *Interview by the author*.

33 **A. Henderson-Sellers** ‘Albedo Changes - Surface Surveillance from Satellites’, *Climatic Change* **2** (1980): 275-81; **A. Henderson-Sellers and N.A. Hughes** ‘Albedo and Its Importance in Climate Theory’, *Prog. Phys. Geog.* **6** (1982): 1-44; **A. Henderson-Sellers and M.F. Wilson** ‘Albedo Observations of the Earth’s Surface for Climate Research’, *Philos. T. Roy. Soc. A* **309** (1983): 285-94. Henderson-Sellers, one of the PhDs from the Leicester group, later became very active in climate modelling. In 1995, she was one of the convening lead authors for the IPCC Second Scientific Assessment report.

34 In 1974, C.D. Rodgers, who was responsible for developing the Met.O.20 GCM’s radiation parameterization, gave a talk at the GARP Study Conference on the Physical Basis of Climate and Climate Modelling. But this was the last time he was directly involved in the discussion of climate modelling. See **C.D. Rodgers** ‘Modelling of Atmospheric Radiation for Climate Studies’, in *The Physical Basis of Climate and Climate Modelling* (Geneva: WMO, 1975): 177-80.

institutions such as the National Center for Atmospheric Research (NCAR) or NASA's research centres. To some extent, this disparity reflected the greater size of the US atmospheric sciences community, which was likely to allow more diversity in climate modelling. Arguably more important, however, was that the issue of climate change, including CO<sub>2</sub>-induced global warming, was politically more salient in the US than in Britain, and that US scientists were generally more responsive than their British counterparts to the political atmosphere surrounding them. GCMs were also accepted as the ultimate tool for climate science in the US, but a number of US scientists were also keen to use RCMs, EBM's or other simple models until GCMs would become more accessible in the future. McGuffie and Henderson-Sellers point out that "in 1980-81, from a total of 27 estimates of the global temperature change resulting from CO<sub>2</sub> doubling, only seven were made by GCMs".<sup>35</sup> Nearly all of these studies came from the US, and the remaining 20 studies were, of course, carried out using RCMs or EBM's.

Hence, while the cultural hierarchy that privileged GCMs was predominant in the two countries, its specific manifestations diverged. And this difference was not only due to processes internal to scientific cultures but related to wider social and political contexts.

A close look at the introduction of the concept of climate model hierarchy reinforces this picture. Such a concept was first formally articulated in the US. In 1971, the SMIC report sought to hierarchically categorize different types of climate models.<sup>36</sup> Later, in 1974, in a review paper on climate modelling, Stephen H. Schneider and Robert E. Dickinson at NCAR extended the idea and presented a more elaborate model classification system based on the geometric degrees of freedom included in the model.<sup>37</sup> The system was called the "hierarchy of climate models". Interestingly enough, the "hierarchy" was not designed to highlight the importance of GCMs. Schneider, the principal author of the review, started his career as a climate scientist by working on a 1-D radiative transfer model to assess the climatic effects of CO<sub>2</sub> and aerosols, and was very supportive of simple climate modelling.<sup>38</sup> Not surprisingly, the paper contended that "the

35 McGuffie and Henderson-Sellers 'Forty Years of Numerical Climate Modelling' , at 1077.

36 SMIC *Inadvertent Climate Modification* . The basic idea was not entirely new. As early as 1952, Jule Charney also proposed an approach "to construct a hierarchy of atmospheric models of increasing complexity". However, his main concern was short-range weather forecasting, and only dynamical models were considered. See Dahan Dalmedico 'History and Epistemology of Models' .

37 S.H. Schneider and R.E. Dickinson 'Climate Modeling', *Rev. Geophys.* **12** (1974): 447-93, at 456.

38 Dickinson took a sceptical reviewer's role in preparing the paper. Schneider *Interview by the author* .

existing or near-future high-resolution GCMs are not the only (or perhaps even the primary) weapons for attacking the problem of long-term climatic variations”.<sup>39</sup> For the future directions of climate modelling, it recommended that “a multiplicity of approaches must be tried and coordinated”.<sup>40</sup>

As I have suggested, therefore, the construction of climate model hierarchy could be seen as an effort to broaden the scope of climate modelling by assigning a meaningful role to simple models. Not unexpectedly, Schneider and Dickinson’s paper faced criticism from GCM scientists. After reading their manuscript, Joseph Smagorinsky, director of the Geophysical Fluids Dynamics Laboratory (GFDL), made a critical comment, saying that simple climate models might have great didactic value, but no more than that.<sup>41</sup> The formulation of this concept, however, was not purely a matter of technical concern. Schneider had a PhD in plasma physics, and from this background, he had acquired advanced numerical techniques. His support for simple models had little to do with the lack of mathematical skills. Schneider also did not share the view of academic meteorologists like Green, and was not sceptical of the GCM approach *per se*. What he found inappropriate was that much energy was devoted to developing the most scientifically elegant models just for science’s sake.<sup>42</sup> He was an outspoken advocate of policy-relevant science.<sup>43</sup> Modelling approaches other than GCMs should be carved out and made use of, he insisted, if that would help tackle potentially serious issues of climate change.

The usefulness of simple climate models continued to be questioned by GCM scientists, but within the next ten years the concept of a climate model hierarchy came to be embraced by many US climate scientists. In Britain, Ann Henderson-Sellers and her post-doc Keith Shine at Liverpool’s geography department proposed an analogous modelling

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39 **Schneider and Dickinson** 'Climate Modeling'.

40 Ibid.

41 **Schneider** *Interview by the author*. Part of this comment was quoted in the review paper. See **Schneider and Dickinson** 'Climate Modeling', at 492-93.

42 **Schneider** *Interview by the author*.

43 See, e.g., **W.W. Kellogg and S.H. Schneider** 'Climate Stabilization - for Better or for Worse', *Science* **186** (1974): 1163-72; **S.H. Schneider** 'Playing Games with the Weather', *New Engineer* **2** (1973): 14-17; 'The Population Explosion - Can it Shake the Climate?' *Ambio* **3** (1974): 150-55; 'Public-Policy Issues in Climatic Change', *EOS T. Am. Geophys. Un.* **57** (1976): 893-94; **S.H. Schneider and R.D. Dennett** 'Climatic Barriers to Long-term Energy Growth', *Ambio* **4** (1975): 65-74; **S.H. Schneider and L.E. Mesirow** *The Genesis Strategy - Climate and Global Survival* (New York: Plenum Press, 1976); **S.H. Schneider and W.M. Washington** 'Energy Production and Climate', *Science* **187** (1975): 13-13

framework. Their academic backgrounds were in astro- and atmospheric physics respectively, and they had both been interested in the use of simple climate models.<sup>44</sup> In 1983, they modified Schneider and Dickinson's classification system by defining the complexity in terms of four elements; radiation, surface processes, dynamics and resolution, and came up with the "climate modelling pyramid".<sup>45</sup> Henderson-Sellers and Shine put more weight on GCMs than Schneider and Dickinson, partly because GCMs had been improved since 1974. This was mirrored in the shape of the pyramid, the apex of which was occupied by GCMs. Still, as in Schneider and Dickinson's model hierarchy, their modelling pyramid gave simple climate models a legitimate role in climate research.

In any case, this scheme did not have much influence on British climate research. Apart from the work of Henderson-Sellers and Shine, there was virtually no discussion of the relations between different modelling approaches. The only active climate modelling group, the Met.O.20, was preoccupied with GCM experiments, maintaining that simple models had only limited value in climate research. This view was also popular among British meteorologists and atmospheric physicists. Moreover, although the societal importance of global climate change was becoming more and more recognized, it was as yet a distant issue for most scientists outside the Met Office or CRU. Both scientifically and politically, there was no incentive to work on simple climate models, and these models were rarely if at all used. The cultural hierarchy thus existed within British climate modelling, centring around GCMs, but differed in nature from that in the US.

## Styles of Global Climate Modelling

A difference of approach also existed in GCM research. In their sociological analyses of the more recent practices of climate change science, Shackley and his colleagues suggest that "epistemic styles" or "epistemic cultures" of global climate modelling can be

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44 Henderson-Sellers began her scientific career by conducting simple modelling studies of the earth's palaeoclimate. See, e.g., **A. Henderson-Sellers** 'Earth's Evolution and Paleoclimatology - Computational Model', *Comput. Geosci.* **4** (1978): 319-31; 'Clouds and the Long-term Stability of the Earth's Atmosphere and Climate', *Nature* **279** (1979): 786-88; **A. Henderson-Sellers and A.J. Meadows** 'Surface Temperature of Early Earth', *Nature* **270** (1977): 589-91. Shine carried out a review study on atmospheric models of varying complexity for his PhD research project at Edinburgh.

45 **K.P. Shine and A. Henderson-Sellers** 'Modeling Climate and the Nature of Climate Models - A Review', *J. Climatol.* **3** (1983): 81-94. Later, Henderson-Sellers further developed this idea in her book, **A. Henderson-Sellers and K. McGuffie** *A Climate Modelling Primer* (Chichester: Wiley, 1987)



identified.<sup>46</sup>

First, Shackley *et al.* assert, there were “thermodynamicists” who paid more attention to the physical basis of the climate system, and there were “dynamicists” who were more oriented towards its dynamical understanding.<sup>47</sup> While the two groups knew very well that modelling climate and its change should deal with both aspects, they had different foci of interest. Secondly, there was a difference between “purists” or “model constructors” who concentrated upon the development of state-of-the-art GCMs, trying to incorporate the full complexity of the climate system, and “pragmatists” or “climate seers” who used GCMs to conduct numerical simulations, especially on anthropogenic climate change.<sup>48</sup> For purists or model constructors, building a higher resolution and more physically-based GCM was their research objective *in itself*; for pragmatists or climate seers, GCM was a tool for climate research, and the evaluation of model performance depended not on its complexity *per se* but on the type of simulation experiments.<sup>49</sup>

As Shackley cautions, his categories of epistemic styles are only “ideal types” based on the observation of global climate modelling of the early 1990s.<sup>50</sup> Though useful, they accordingly have some limitations in fully capturing the characteristics of GCM practices during the period from the late 1950s to the mid 1980s as examined in the previous chapters. In the early phase of GCM research, for example, the priority was inevitably given to the model development, and in most cases, model constructors and users were the same scientists. George Corby and Andrew Gilchrist at the Met.O.20 and Joseph Smagorinsky and Syukuro Manabe at GFDL were all closely engaged in the formulation

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46 **S. Shackley et al.** 'Adjusting to Policy Expectations in Climate Change Modeling - An Interdisciplinary Study of Flux Adjustments in Coupled Atmosphere-Ocean General Circulation Models', *Climatic Change* **43** (1999): 413-54. For discussion on styles of science, see, e.g., **L. Fleck** *Genesis and Development of a Scientific Fact* (Chicago: University of Chicago Press, 1979); **J.S. Fruton** *Contrasts in Scientific Style: Research Groups in the Chemical and Biochemical Sciences* (Philadelphia: American Philosophical Society, 1990); **J.H. Fujimura and D.Y. Chou** 'Dissent in Science - Styles of Scientific Practice in the Controversy over the Cause of Aids', *Soc. Sci. Med.* **38** (1994): 1017-36; **J. Harwood** *Styles of Scientific Thought: The German Genetics Community, 1900-1933* (Chicago: University of Chicago Press, 1993); **J. Maienschein** 'Epistemic Styles in German and American Embryology', *Science in Context* **4** (1991): 407-27; **M.J. Nye** 'National Styles - French and English Chemistry in the 19<sup>th</sup> and Early 20<sup>th</sup> Centuries', *Osiris* **8** (1993): 30-52; **M.J.S. Rudwick** 'Cognitive Styles in Geology', in *Essays in the Sociology of Perception*, M. Douglas (ed.) (London: Routledge, 1982): 210-42.

47 **S. Shackley** 'Epistemic Lifestyles in Climate Change Modeling', in *Changing the Atmosphere - Expert Knowledge and Environmental Governance*, C.A. Miller and P.N. Edwards (eds.) (Cambridge: MIT Press, 2001): 107-34; **Shackley et al.** 'Adjusting to Policy Expectations in Climate Change Modeling'.

48 **Shackley** 'Epistemic Lifestyles'; **Shackley et al.** 'Adjusting to Policy Expectations in Climate Change Modeling'.

49 Ibid.

50 **Shackley** 'Epistemic Lifestyles'.

of the model's dynamical structure and physical parameterizations as well as the testing of the model in climate simulations. These and other GCM scientists basically had a dynamicist bias. They had a disciplinary background in dynamical meteorology, and viewed GCMs as not much more than a natural extension of numerical weather prediction models.

Despite the commonalities noted above, each GCM group had a different set of research questions, technical interests and institutional mission, and was located in a different organizational culture and national political atmosphere. Individual scientists also often had distinct preferences. These factors had some impact on the way in which the models were developed and utilized, giving rise to distinct cultures of global climate modelling.

Throughout the 1960s and 1970s, the Met.O.20 was the only GCM research group that was a direct branch of the national meteorological service – *i.e.*, the Met Office. GFDL was also created under the US National Weather Service, but was almost an independent laboratory specializing in GCM research. At the Met Office, short-range numerical weather forecasts were produced operationally by the Central Forecasting Office. Within the deputy directorate of dynamical research, numerical weather prediction (NWP) and global climate models were being developed simultaneously. Although the NWP work was undertaken independently at the Forecasting Research Branch,<sup>51</sup> the Met.O.20's GCM research was shaped in crucial ways by the fact that the primary task of the Office was operational weather forecasting.

The first generation of the Met.O.20 scientists – George Corby, Andrew Gilchrist, Roger Newson, Peter Rowntree and others – could be said to be dynamicists. They were mathematicians by background, and were trained as dynamical meteorologists at the Met Office College and in-house.<sup>52</sup> Most of them worked for several years either at the Forecasting Research Branch or at the Central Forecasting Office before joining the Met.O.20. Shackley *et al.* argue that, in the early 1990s, dynamicists from weather

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51 In the early to mid 1950s, the Met Office had the Forecasting Research Division, consisting of Short-range and Long-range Forecasting Branches. This division was reorganized into the Dynamical Research Branch in the late 1950s and then into the Forecasting Research Branch under the deputy directorate of dynamical research in the mid 1960s. See Anon. 'Reorganization of Assistant Directorates', *Meteorol. Mag.* **94** (1965): 33-34; **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1951-69)

52 For the Met Office College, see. **D.H. Johnson** 'Meteorological Office College', *Meteorol. Mag.* **101** (1972): 299-305; **C.J. Readings and P.G. Wickham** 'The Meteorological Office College, 1972-86', *Meteorol. Mag.* **115** (1986): 220-28

forecasting and NWP were likely to be more purist-oriented and would have ruled out using low-resolution GCMs with crude physical parameterizations.<sup>53</sup> The Met.O.20 scientists were likewise reluctant to use simpler GCMs. Having been influenced by the forecasting context, they were more conscious of regional scale variations and wanted to secure the model's ability to reproduce them. This does not mean that their modelling style was that of a purist. The Met.O.20 scientists were rather pragmatic: the use of dynamical methods was guided by whether it would have practical use for the intended purposes.

And the intended purposes, conversely, reflected the Met Office's forecasting context. The Met.O.20's major research concern at that stage was the development of GCMs that could realistically simulate short-term and regional variations of the observed climate. While GCM research groups elsewhere all pursued the same goal, it was taken as particularly imperative by the Met.O.20 scientists. This was in part a response to the institutional setting in which the Met.O.20 was located. As mentioned, one of the early rationales for establishing the Met.O.20 was to explore and develop the possibility of dynamical long-range forecasting for about a month ahead or even for seasons. The emphasis on short-term variations of the natural climate was therefore to be expected. By the mid 1970s, the Met.O.20's research focus shifted towards the simulation of anthropogenic or long-term climate changes.<sup>54</sup> For quite some time, however, the model's physical realism of this type remained an essential constituent of the Met.O.20's GCM practices.

This raises an issue of organizational culture. That the Met Office continuously issued weather forecasts to government agencies, industry and the public, which might have significant societal consequences, brought in a certain culture of conservatism and pragmatism. The Met.O.20 scientists enjoyed an academic-like environment,<sup>55</sup> but they were cautious in changing their style of doing things and in adopting new theories or techniques even when they thought there were scientific reasons for the changes. As part of the British Scientific Civil Service and of the Air Ministry, the Office also had a

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53 **Shackley et al.** 'Adjusting to Policy Expectations in Climate Change Modeling'.

54 See Chapters 3 and 4. See also below.

55 This was especially the case for junior scientific staff who had joined the Met Office in the early to mid 1970s.

**J.M. Slingo** *Interview by the author* (October 7, 1999)

relatively hierarchical organizational culture. The Met.O.20's research priorities and activities were controlled at the level of the directorate of research and the deputy directorate of dynamical research, and through this process, a conservative and pragmatic style of numerical modelling was strengthened. As the model development matured, the Met.O.20 grew in size and scope, increasing the number of junior scientists. Yet they were under strong supervision by the assistant director of the Met.O.20 and by senior scientists. The potential diversity of research interests and practices within the Met.O.20 thus tended to be suppressed.

One difficulty in GCM research was that a balance routinely had to be struck between the model's vertical and horizontal resolution, the intricacy of physical parameterization schemes, and the length of numerical integration. Even with the rapid advance in technology, GCM scientists never felt that they had enough computing resources. The computing capability of the Met.O.20 was far better than that of university research groups, but was more limited than that of GFDL. The branch could only use the computer after the requirements for operational forecasting and NWP model developments were fulfilled. Though its computer was powerful by the standards of the time and was regularly upgraded and replaced,<sup>56</sup> the Office could not provide the computing power and time that the Met.O.20 scientists wanted. Only in 1982, when a new Cyber 205 supercomputer was installed, did the Met.O.20 initiate a full-fledged effort to model global climate change.

All these features acted as constraints and resources for the Met.O.20's model design and choice of parameterization schemes. Three examples could illustrate this point. First, in selecting a radiative transfer scheme for its first GCM, the Met.O.20 opted for a non-interactive scheme based on climatologically specified cooling and heating rates.<sup>57</sup> The Met.O.20 scientists believed that this approach was not suitable for modelling climate

<sup>56</sup> For the Met Office computers from the late 1950s to the early 1980s, see the following table.

Year	Computer	Calculations per second	Main memory (words)	Horizontal resolution for the main NWP model used (global/local) / vertical levels
1959	Ferranti Mercury	$3 \times 10^3$	$1 \times 10^3$	(N.A./320km) / 2 levels
1965	English Electric KDF 9	$50 \times 10^3$	$12 \times 10^3$	(N.A./300km) / 3 levels
1972	IBM 360/195	$4 \times 10^6$	$250 \times 10^3$	(300km/100km) / 10 levels
1982	CDC Cyber 205	$200 \times 10^6$	$1 \times 10^6$	(150km/75km) / 15 levels

From <http://www.meto.gov.uk/research/nwp/numerical/computers/history.html>. See also **M.K. Hinds** 'Computer Story', *Meteorol. Mag.* **110** (1981): 69-81.

perturbations due to external forcings such as solar irradiation changes or varying concentrations of atmospheric gases, and in turn, for modelling long-term or anthropogenic climate changes. The selected scheme was nevertheless computationally efficient and gave a better simulation of the observed climate. Secondly, given that dynamical ocean circulation models were not yet available, the Met.O.20 chose to prescribe the sea surface temperatures in their GCM.<sup>58</sup> As a result, the oceans were assumed to have an infinite thermal capacity, giving no response in the perturbed simulations. But since the prescribed data contained the varying effects of oceanic heat and sea-ice distribution, the Met.O.20 scientists claimed, this method had an advantage in simulating seasonal and regional variations of the natural climate. Thirdly, whereas some GCM groups decided to use spectral methods as their numerical technique in the mid 1970s, the Met.O.20 was opposed to its introduction.<sup>59</sup> Spectral methods were thought to make improvements in speed and treating dynamics, and appealed to both those who wanted to run long-term integrations and those who were working on dynamical processes that were larger than usually included in climate simulations.<sup>60</sup> The Met.O.20 had already committed to finite-difference methods, and was unwilling to change the model's numerical structure. Besides, spectral models at the time gave good results only for low-resolution, and seemed incompatible with the detailed simulation of natural climate variability.

To summarize: the style of the Met.O.20's general circulation modelling in the 1960s to mid 1970s was broadly dynamicist in character. This style was also pragmatic, but in a different sense from Shackley's pragmatists or climate seers. The Met.O.20 scientists had rigorous standards for the model's ability to simulate short-term and regional variations of the present climate, and considered it a prerequisite for any further climate simulation experiments. Unlike "purist-dynamicists" in Shackley *et al.*'s study,<sup>61</sup> however, these standards were informed by the practical context of the Met Office rather than purely

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57 See previous chapters.

58 Ibid.

59 **A. Gilchrist** *Interview by the author* (June 9, 1999). See also **J.T. Houghton and P.W. White** 'Retirement of Mr A. Gilchrist', *Meteorol. Mag.* **118** (1989): 21-22. For spectral methods in the 1970s, see **W. Bourke et al.** 'Global Modelling of Atmospheric Flow by Spectral Methods', in *General Circulation of the Atmosphere*, J. Chang (ed.), *Methods in Computational Physics*, Vol. 17 (New York: Academic Press, 1977): 267-324.

60 **A. Slingo** *Interview by the author* (October 8, 1999). For the UK Universities modelling group, see **R.P. Pearce and B.J. Hoskins** 'Advances in Atmospheric Circulation Studies', *NERC News* **1** (1987): 5-9.

61 **Shackley et al.** 'Adjusting to Policy Expectations in Climate Chance Modeling'.



dynamical concerns.

This unique style of climate modelling was quite different from those of other GCM research centres. At GFDL, for instance, modellers of the climate seer-type seem to have appeared early on, even before the division of labour between model users and constructors became apparent. GFDL was larger than the Met.O.20, with more research scientists and its own computing facilities. The institutional mission of GFDL was to develop diverse potential climate research applications for GCMs.<sup>62</sup> Long-term or anthropogenic climate changes were as important as short-term climate variations. There was thus room for those who were more interested in examining climate sensitivity and feedback than in refining GCMs. The leading scientist of this group was Manabe. The model resolution, the complexity of physical parameterizations, and the model's ability to reproduce short-term and regional variations of the natural climate were also central to him, but not to the extent that concerns about these elements would prevent him from conducting climate change simulations using the current model.

Manabe's climate seer style was partly the outcome of his individual choice and GFDL's broad institutional goals, but also partly shaped by US political interest in the possibility of anthropogenic climate change. Although, by 1961-62, Manabe was aware of the global warming hypothesis through collaboration with Fritz Möller, he was busy developing a radiative transfer scheme for GFDL's GCM.<sup>63</sup> The turning point came a few years later when the National Academy of Sciences (NAS) Panel on Weather and Climate Modification requested GFDL to investigate the effects of atmospheric CO<sub>2</sub> and other anthropogenic gases on global climate.<sup>64</sup> Manabe was assigned to take up the task. He modified his radiative transfer scheme to perform RCM experiments on the topic. This incident gave impetus to his subsequent and more refined RCM and GCM studies of the greenhouse effect.<sup>65</sup> Manabe's use and development of GCMs were then framed by his

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62 See, e.g., **J. Smagorinsky** 'Numerical Simulation of the Global Atmosphere', in *The Global Circulation of the Atmosphere*, G.A. Corby (ed.) (London: Royal Meteorological Society, 1969): 24-41; 'The Beginnings of Numerical Weather Prediction and General Circulation Modeling - Early Recollections', *Adv. Geophys.* **25** (1983): 3-37.

63 **Manabe** Interview by P.N. Edwards .

64 **J. Smagorinsky** Interview by S.R. Weart (AIP Oral History Project: March 1, 1989); **US NRC Weather and Climate Modification - Problems and Prospects, Vol. 2: Research and Development** (Washington, DC: National Academy of Sciences, 1966)

65 See, e.g., **S. Manabe** 'Estimates of Future Change of Climate Due to the Increase of Carbon Dioxide Concentration in the Air', in *Man's Impact on the Climate*, W.H. Matthews et al. (eds.) (Cambridge: MIT Press, 1971): 249-64; **S. Manabe and R.T. Wetherald** 'Thermal Equilibrium of Atmosphere with a Given Distribution

research aim and related technical concerns; that is, a low-resolution GCM with relatively simple physical parameterizations was tolerated as long as the radiative aspects of the model were sophisticated enough to assess the overall climatic effects of increased CO<sub>2</sub>. The same argument can be applied to the climate seer-like modelling style of James Hansen's team at NASA Goddard Institute of Space Studies (GISS), who began GCM research in the late 1970s specifically to address the problem of anthropogenic climate change.<sup>66</sup>

The Met.O.20 also commenced its CO<sub>2</sub> research group in the late 1970s. The scientist responsible for GCM simulations of CO<sub>2</sub>-induced climate change was John Mitchell, who held a PhD in physics. He was more of a model user than a model constructor, with a perspective resembling that of Manabe or Hansen, and was more concerned with the physical mechanisms of climate change than with reproducing the presently observed climate.<sup>67</sup> As noted above and in the previous chapters, though, the Met.O.20's GCM practice did not change immediately. A cautious modelling style persisted, and the model's physical realism that emphasized seasonal and regional variations of the natural climate remained an important concern, even in doubled CO<sub>2</sub> simulation experiments. Furthermore, as the Met.O.20 increased its climate modelling efforts, a division of labour emerged between model users and constructors. Among the latter group, those working on physical parameterizations were especially critical of using GCMs that would not properly represent cloud-radiation interactions and cloud feedback.<sup>68</sup> While the Met.O.20's GCM work on the CO<sub>2</sub> effect was led by climate seer-type scientists, there was therefore a potential tension between different scientific styles. Yet in the prevailing hierarchical culture, senior scientists could rather effectively harmonize these differences. The resulting hybrid style of climate modelling allowed the Met.O.20 to implement a coherent CO<sub>2</sub> research programme,<sup>69</sup> providing the basis for future research at the Hadley Centre

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of Relative Humidity', *J. Atmos. Sci.* **24** (1967): 241-59; 'Effects of Doubling CO<sub>2</sub> Concentration on Climate of a General Circulation Model', *J. Atmos. Sci.* **32** (1975): 3-15

66 For a brief history of Hansen's group, see **J. Hansen et al.** 'Climate Modeling in the Global Warming Debate', in *General Circulation Model Development - Past, Present, and Future*, D.A. Randall (ed.) (San Diego: Academic Press, 2000): 127-64.

67 **J.F.B. Mitchell** *Interview by the author* (October 8, 1999)

68 **Slingo** *Interview by the author*.

69 Shackley argues that later in the 1990s the Hadley Centre adopted "the hybrid climate modeling-policy style – in which the policy-influenced objectives and priorities of the research organization, as defined by its leadership, take precedence over other individual or organizational motivations and styles". See {Shackley, 2001 #1601}. It seems that, even before policy considerations became prominent, a hierarchical organizational culture of the

for Climate Prediction and Research.

## Different Paths of Global Warming Research

As briefly reviewed in Chapter 1, the idea of the enhanced greenhouse effect has a long history that can be traced back to the late 19<sup>th</sup> century. As early as the 1930s, a global warming hypothesis which was close to the current form could be found in meteorological journals.<sup>70</sup> The issue then faded in the next few decades, though it continued to be debated and studied among a few groups of scientists. By the mid to late 1970s, however, the issue of the connection between climate and atmospheric CO<sub>2</sub> revived, attracting considerable interest from the broad international climate science community.

The evolution of global warming research was likely to be a slow and circuitous process. As in many other geophysical investigations, the scale and complexity of the problem was such that long-term global observation was not easily attainable, nor were controlled laboratory experiments feasible. Research into global warming thus not only demanded a lengthy compilation, processing and analysis of climatological and other relevant data, but also required the use of large-scale computer simulations as its integral part. One may expect that, if the global warming hypothesis were to receive sufficient recognition, empirical evidence should be accumulated and state-of-the art computer models (*e.g.*, GCMs) be developed, and that such processes would take longer than in usual laboratory sciences.

But recent historical and social studies of climate science suggest that the trajectory of global warming research was not simply driven by the internal logic of science.<sup>71</sup> Weart,

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Met Office led to a hybrid modelling style.

70 See, *e.g.*, **G.S. Callendar** 'The Artificial Production of Carbon Dioxide and Its Influence on Temperature', *Q. J. Roy. Meteor. Soc.* **64** (1938): 223-37.

71 For the standard account of the history of global warming science, see, *e.g.*, **J.H. Ausubel** 'Appendix 2: Historical Note', in *Changing Climate - Report of the Carbon Dioxide Assessment Committee* (Washington, DC: National Academy of Sciences, 1983): 488-91; **R.G. Fleagle** 'From the International Geophysical Year to Global Change', *Rev. Geophys.* **30** (1992): 305-13; **J. Jäger and T. O'Riordan** 'The History of Climate Change Science and Politics', in *The Politics of Climate Change - A European Perspective*, T. O'Riordan and J. Jäger (eds.) (London: Routledge, 1996): Chapter 1; **M.D.H. Jones and A. Henderson-Sellers** 'History of the Greenhouse Effect', *Prog. Phys. Geog.* **14** (1990): 1-18; **W.W. Kellogg** 'Mankind's Impact on Climate - The Evolution of an Awareness', *Climatic Change* **10** (1987): 113-36; **R. Revelle** 'Introduction - The Scientific History of Carbon Dioxide', in *The Carbon Cycle and Atmospheric CO<sub>2</sub> - Natural Variations, Archean to Present*, E.T. Sundquist and W.S. Broecker (eds.), Geophysical Monograph, No. 32 (Washington, DC: American Geophysical Union, 1985): 1-4.

for example, reveals that US atmospheric CO<sub>2</sub> research in the 1950s was a product of a number of disconnected projects, such as those concerned with spectrophotometry, atmospheric infrared transmission, or radioactive substances, all of which benefited from the Cold War environment.<sup>72</sup> According to him, public anxieties about nuclear weapons might also have encouraged people to envisage the possibility that humans could inadvertently alter the global environment.<sup>73</sup> In a similar vein, Hart and Victor show that, in the US from the late 1950s through the mid 1970s, global warming research progressed when scientific elites involved were able to identify and take advantage of “policy windows” that could connect their ongoing research to the political issues of the time (*e.g.*, inadvertent weather modification or environmental pollution).<sup>74</sup> From a slightly different angle, Edwards describes how the developments of GCMs, global meteorological observing networks and technologies for data collection and processing (*e.g.*, satellites and electronic computers) were all intertwined with Cold War and international environmental politics.<sup>75</sup>

Whether broad social and political contexts outside science would favour or disfavour the development of global warming research was, of course, a contingent matter. This is well illustrated by the differences in the path of global warming research between the US and Britain, and within Britain.

Public concern about fallout from atmospheric nuclear bomb tests or nuclear reactor accidents did exist in Britain in the 1950s and early 1960s, raising scientific interest in the meteorological aspects of atmospheric trace substances. In collaboration with the World Meteorological Organization (WMO), the Met Office’s Special Investigations Branch made atmospheric radioactivity measurements during the International Geophysical Year (IGY), 1957-58.<sup>76</sup> When there was a fire at the Windscale plutonium reactor in 1957, the Branch produced an analysis of the transport of radioactive pollution for the United

72 **S.R. Weart** ‘Global Warming, Cold War, and the Evolution of Research Plans’, *Hist. Stud. Phys. Biol.* **27** (1997): 319-56; ‘Government: The View from Washington, DC’, in *The Discovery of Global Warming*, <http://www.aip.org/history/climate> (recorded copy at American Institute of Physics, 2001): p. USGovernment-1-25 of 08/01/01 text.

73 **S.R. Weart** ‘The Public and Climate’, in *The Discovery of Global Warming*, <http://www.aip.org/history/climate> (recorded copy at American Institute of Physics, 2001): p. Public-15 of 08/01/01 text.

74 **D.M. Hart and D.G. Victor** ‘Scientific Elites and the Making of US Policy for Climate Change Research, 1957-74’, *Soc. Stud. Sci.* **23** (1993): 643-80.

75 **P.N. Edwards** ‘The World in a Machine - Origins and Impacts of Early Computerized Global Systems Models’, in *Systems, Experts, and Computers*, T.P. Hughes and A.C. Hughes (eds.) (Cambridge: MIT Press, 2000)

76 **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1957-59)

Kingdom Atomic Energy Authority.<sup>77</sup> However, these activities were not followed by either the kind of radiocarbon exchange studies being carried out by Revelle and Suess in the US or the type of carbon cycle modelling by Bolin and Eriksson in Sweden. There were a few British scientists such as a geochemist Alan Walton, who had been involved in US global carbon cycle research, but on returning to Britain they turned their research away from the subject.<sup>78</sup>

The fear of nuclear fallout was soon succeeded by more general concerns about weather and climate modification and atmospheric pollution. The US scientific community actively participated in, and facilitated, this process. The ensuing discussions created a “policy window” for linking atmospheric modelling to greenhouse effect research. In 1965-66, the Environmental Pollution Panel of the President’s Science Advisory Committee (PSAC) and the NAS Panel on Weather and Climate Modification each prepared reports of their findings and recommendations. As shown in Chapter 4, Smagorinsky and Manabe of GFDL were engaged in both the PSAC’s CO<sub>2</sub> sub-panel chaired by Revelle and the NAS Panel. The result was the first RCM simulation of CO<sub>2</sub>-induced global warming.<sup>79</sup> By contrast, British meteorologists and atmospheric physicists showed little interest in the possibility of altering large-scale atmospheric conditions by human activities, whether deliberate (*e.g.*, hydraulic engineering projects) or inadvertent (*e.g.*, increased atmospheric CO<sub>2</sub> levels). The Met.O.20 scientists often mentioned these problems, but only to underline the importance of GCM developments.<sup>80</sup>

In the late 1960s and early 1970s, a series of climatic extremes were reported to occur around the world. Most climate scientists believed that these episodes were natural in origin, although whether they should be interpreted in terms of random climate fluctuations or of longer-term climate variations was contested.<sup>81</sup> Yet the reports of many climatic extremes within a short period tended to divert more attention to the argument

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<sup>77</sup> Ibid.

<sup>78</sup> For some of Walton’s early work, See, *e.g.*, **W.S. Broecker and A. Walton** ‘The Geochemistry of C-14 in Fresh-Water Systems’, *Geochim. Cosmochim. Ac.* **16** (1959): 15-38; ‘Radiocarbon from Nuclear Tests’, *Science* **130** (1959): 309-14; **A. Walton et al.** ‘Studies of Radioactive Fallout Following 1961 USSR Tests’, *Nature* **199** (1963): 526-28.

<sup>79</sup> **US NRC** *Weather and Climate Modification*, Vol. 2

<sup>80</sup> See, *e.g.*, **British Met Office** *Annual Report of the Meteorological Office* (London: HMSO, 1965-70)

<sup>81</sup> See, *e.g.*, **H.H. Lamb** *The Current Trend of World Climate. A Report on the Early 1970s and a Perspective*, CRU Research Publication, No. 3 (Norwich: University of East Anglia, 1974); *Climate: Present, Past and Future* - Vol. 2 *Climatic History and the Future* (London: Methuen, 1977)



that there might be additional causes outside natural climate variability. This was also the time when environmentalism emerged as an international political force, as symbolized by the 1972 UN Conference on the Human Environment in Stockholm. The idea of human intervention in nature on a large scale appeared to be more plausible than ever, both to the general public and to scientists. The workshops SCEP and SMIC, and the USC-GARP Panel on Climate Variation, were all organized in this historical context, and served as a catalyst for debate on the global warming hypothesis in the US.<sup>82</sup>

Again, Britain responded differently, passing by this window of opportunity for global warming research. Mick Kelly later recalled that the first wave of environmental awareness in the late 1960s and early 1970s “had very little effect” on the way British climate science was organized and conducted.<sup>83</sup> The perceived increase in climatic disasters world-wide also did not lead to scientific concerns about the greenhouse effect.

Hubert Lamb and his colleagues at CRU, who themselves had contributed to growing climate concern, kept focusing on climate history and natural climate changes. For them, CO<sub>2</sub>-induced global warming was only one of the many topics that could demonstrate the need for the kind of observational work they were doing.<sup>84</sup> Mainstream meteorologists and atmospheric physicists acknowledged the potential climatic effects of anthropogenic CO<sub>2</sub> emissions, but on their part they did not see any immediate reason to initiate research into global warming. In 1971, the Royal Commission on Environmental Pollution (RCEP) published its first report. The Met Office scientists assisted RCEP to prepare the “Global Effects of Atmospheric Pollution” section in the report. The conclusion of the section, reflecting the view of the Met Office senior scientists, was quite modest, and more significantly, did not recommend any change in British climate research.<sup>85</sup>

The difference between the US and Britain continued. In the US, beginning in the mid 1970s, in the wake of concerns about the uncertain energy future, global warming began

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82 **SCEP** *Man's Impact on the Global Environment*; **SMIC** *Inadvertent Climate Modification*; **USC-GARP** *Understanding Climatic Change - A Program for Action* (Washington, DC: National Academy of Sciences, 1975)

83 **P.M. Kelly** *Interview by the author* (August 4, 1999)

84 The other three aims were; 1) To establish firmer knowledge of the history of climate in the recent and distant past; 2) To monitor and report on current climatic developments on a global scale; and 3) To identify the processes (natural and man-made/anthropogenic) at work in climatic fluctuations and the characteristic time-scales of their evolution. **Climatic Research Unit**, *Monthly Bulletin* 1 (1972)

85 **RCEP** 'Chapter V. Global Effects of Atmospheric Pollution', in *First Report of the Royal Commission on Environmental Pollution* (London: HMSO, 1971): 36-43.

to be framed as a challenge to the management of energy resources.<sup>86</sup> While such a framing of the issue, as Clark and Dickson argue, might have limited the scope of the US debate on the greenhouse effect,<sup>87</sup> it provided a secure context in which global warming research could be expanded. Among others, the US Department of Energy (USDOE) became a key actor, funding and coordinating a wide range of research projects related to atmospheric CO<sub>2</sub>. In Britain, the two successive oil shocks and the nuclear power controversy were less of an issue than in the US, even though they had broad influence in society at large. As we have seen, CO<sub>2</sub>-induced global warming was rarely approached as an energy problem. The government showed virtually no interest in the subject.

The Met Office also gradually moved towards global warming research over the course of the mid to late 1970s, but not so much as a response to growing energy concerns. More important in the Met Office's transition were a range of WMO-led research initiatives into climate modelling and atmospheric CO<sub>2</sub> – for example, the GARP Study Conference on the “Physical Basis of Climate and Climate Modelling” in 1974, where Manabe presented the first GCM study of the greenhouse effect, the WMO Scientific Workshop on Atmospheric CO<sub>2</sub> in 1976, the instigation of the WMO Research and Monitoring Project on Atmospheric CO<sub>2</sub> in 1977, and others.<sup>88</sup> The Met Office was not directly involved in all these activities, but given its close institutional link with the WMO, these events were influential in persuading the Office scientists that CO<sub>2</sub>-induced global warming deserved careful study. And yet, at this stage, the Met Office scientists restricted their discussion on the CO<sub>2</sub> issue mainly to the context of GCM developments.

Outside the Met Office, though, the reframing of the CO<sub>2</sub> issue in the US had some

86 For the concept of “framing”, see **C.A. Miller** ‘The Dynamics of Framing Environmental Values and Policy: Four Models of Societal Processes’, *Environ. Value* 9 (2000): 211-33.

87 **W.C. Clark and N.M. Dickson** ‘Civic Science: America's Encounter with Global Environmental Risks’, in *Learning to Manage Global Environmental Risks*, Social Learning Group (ed.) (Cambridge: MIT Press, 2001): 259-94.

88 **R.D. Bojkov** ‘Summary of Some WMO Activity Relevant to Atmospheric Carbon Dioxide’, in *Proceedings of the Carbon Dioxide Research Conference - Carbon Dioxide, Science and Consensus*, S.M. Kincade (ed.) (Washington, DC: US Department of Energy, 1982): A27-A38; **GARP** *The Physical Basis of Climate and Climate Modelling - Report of the International Study Conference*, GARP Publication Series, No. 16 (Geneva: WMO, 1975); **WMO** *Report of the Scientific Workshop on Atmospheric Carbon Dioxide*, WMO series, No. 474 (Geneva: WMO, 1977). WMO also hosted a number of workshops and conferences on the climatic effects of CO<sub>2</sub>, often together with other international agencies. See, e.g., **J. Williams** (ed.) *Carbon Dioxide, Climate and Society - Proceedings of a IIASA Workshop* (Oxford: Pergamon Press, 1978); **WMO** *Meeting of Experts on the Use of Carbon Isotopes in Atmospheric CO<sub>2</sub> Research*, WMO Project on Research and Monitoring of Atmospheric CO<sub>2</sub> Report, No. 1 (Geneva: WMO, 1979); *Report of the Meeting of CAS Working Group on Atmospheric Carbon Dioxide*, WMO Project on Research and Monitoring of Atmospheric CO<sub>2</sub> Report, No. 2 (Geneva: WMO, 1979); **WMO/UNEP/ICSU** *Proceedings of the World Climate Conference - A Conference of*

indirect impact on British climate science. It was USDOE that, since the mid 1970s, had been the major funding body for CRU's project to produce the global land-based temperature data set. From USDOE's perspective, the issue of global warming was behind the background from the very beginning, but under the directorship of Hubert Lamb, who was sceptical of the idea, this did not become explicit. The situation was to be changed when Lamb retired and was replaced by Tom Wigley in 1978. Wigley felt that global warming could be an important "publicity source" for CRU.<sup>89</sup> Capitalizing on funding from USDOE, he pushed the Unit further to global warming research. In addition to the global temperature data set, CRU began to take up more policy-relevant research questions such as the detection of CO<sub>2</sub>-induced climate change and the construction of warming scenarios based on past climatic records.<sup>90</sup>

These differences in the path of global warming research were closely related to the diversity of scientific subcultures and styles that I have described above. In contrast to the recent focus on GCMs, well into the late 1970s and even into the early 1980s, approaches other than GCMs had played a more important role in the development of global warming research – for example, the analysis of observational climate data by J. Murray Mitchell, carbon cycle modelling by Bert Bolin, simple climate modelling studies by Stephen Schneider, Syukuro Manabe or James Hansen. In Britain, however, these research streams were almost absent. Hubert Lamb did carry out observational studies of climate, but was more concerned with natural climate forcings such as volcanic eruptions. British meteorologists and atmospheric physicists did study radiation, but never paid serious attention to the development of physical climate models. The Met Office scientists maintained that climate and its change could only be properly studied using GCMs. Even when wider social and political contexts were potentially favourable for the development of global warming research, therefore, these opportunities were not seized in Britain.

As discussed above, the Met Office scientists did not just embrace GCMs as a principal methodology. The use of EBMs, RCMs or other type of simple models was simply not an

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*Experts on Climate and Mankind*, WMO series, No. 537 (Geneva: WMO, 1979)

89 **T.M.L. Wigley** *Interview by the author* (August 14, 2000)

90 **T.M.L. Wigley** 'The Impact of Future Atmospheric CO<sub>2</sub> Increases', in *Workshop on Environmental and Societal Consequences of a Possible CO<sub>2</sub>-Induced Climate Change* (Washington, DC: US Department of Energy, 1980): 141-47; **T.M.L. Wigley and P.D. Jones** 'Detecting CO<sub>2</sub>-Induced Climatic Change', *Nature* **292** (1981): 205-08; **T.M.L. Wigley et al.** 'Scenario for a Warm, High-CO<sub>2</sub> World', *Nature* **283** (1980): 17-21.

option for them. They also developed a style of general circulation modelling that particularly emphasized the model's ability to simulate short-term and regional variations of the natural climate. This modelling philosophy, which, I have argued, was partly shaped by the institutional goal of the Met Office, tended to presuppose more stringent model requirements for GCM simulation of CO<sub>2</sub>-induced climate change than those adopted by US counterparts. This may not have necessarily discouraged the Met.O.20 scientists from pursuing global warming research, but resulted in a rather different research focus – for instance, regional climate consequences of anthropogenic CO<sub>2</sub> increase.

The Met.O.20's cautious, GCM-centred approach was also connected to a rather old-fashioned, conservative attitude of the Met Office senior scientists towards the role of science in policy; that is, science and policy were strictly demarcated, and only a few senior staff were allowed to deal with policy matters. "Science" usually meant that GCM-based science. Policy discussions based on simple models or empirical methods were seen as unreliable. In the US, the meteorological community seems to have been relatively more willing to discuss the policy aspect of their research. More importantly, there were a number of scientist-advocates such as William Kellogg, Stephen Schneider, and James Hansen, who not only sought societal support for climate science but also actively attempted to address the issues in policy arena. While these scientists largely accepted the cultural hierarchy of climate science, centred around GCMs, they had a more flexible and sympathetic approach to simple climate modelling or observational studies.

Thus, the science of global warming was by no means homogeneous. Different institutional goals, different national political environments, different understandings of how to relate to policy, and the hierarchical relations between scientific subcultures led to rather different paths and styles of global warming research.

## List of Oral History Interviews

<b>R.A. Bryson</b>	(April 27, 2000)
<b>G.A. Corby</b>	(April 10, 2000)
<b>M.J.P. Cullen</b>	(July 26, 2000)
<b>C.K. Folland</b>	(October 5, 1999)
<b>W.L. Gates</b>	(March 27, 2001)
<b>A. Gilchrist</b>	(June 9, 1999)
<b>R.M. Goody</b>	(January 12, 2001: <i>personal communication</i> )
<b>J.S.A. Green</b>	(August 3, 1999)
<b>R.S. Harwood</b>	(February 25, 1999)
<b>J.T. Houghton</b>	(October 4, 1999)
<b>P.D. Jones</b>	(August 15, 2000)
<b>A. Kasahara</b>	(May 9, 2001)
<b>P.M. Kelly</b>	(August 4, 1999)
<b>S. Manabe</b>	(August 28, 2000: <i>telephone interview</i> )
<b>B.J. Mason</b>	(July 5, 1999)
<b>J.F.B. Mitchell</b>	(October 8, 1999)
<b>R.L. Newson</b>	(August 2, 2000)
<b>D.E. Parker</b>	(October 5, 1999)
<b>C.D. Rodgers</b>	(May 19, 2000)
<b>P.R. Rowntree</b>	(October 4, 1999)
<b>S.H. Schneider</b>	(January 23, 2001)
<b>A. Slingo</b>	(October 8, 1999)
<b>J.M. Slingo</b>	(October 7, 1999)
<b>C. Tickell</b>	(July 27, 1999)
<b>T.M.L. Wigley</b>	(August 14, 2000)



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